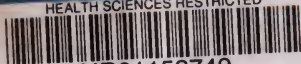


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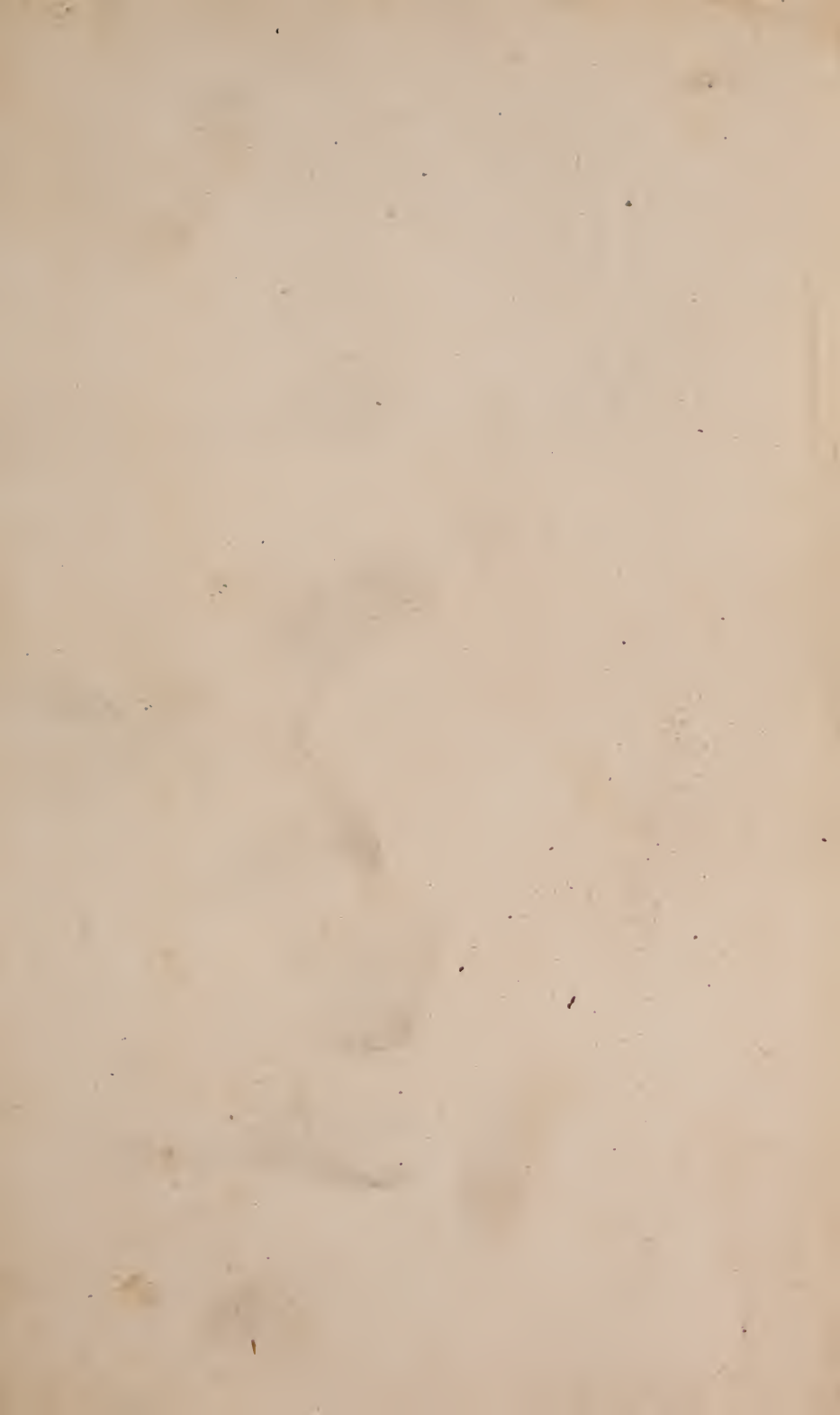
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Archives of Medicine

A BI-MONTHLY JOURNAL

DEVOTED TO ORIGINAL COMMUNICATIONS ON MEDICINE,
SURGERY, AND THEIR SPECIAL BRANCHES

EDITED BY

E. C. SEGUIN, M. D.

S'il est possible de perfectionner l'espèce
humaine, c'est dans la médecine qu'il faut
en chercher les moyens.

—DESCARTES

SEVENTH VOLUME

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ARCHIVES OF MEDICINE.

Original Articles.

CARCINOMATOUS PAPILLOMA OF BLADDER. OCCLUSION OF ORIFICES OF URETERS. DEATH BY URÆMIC COMA.

By SAMUEL B. WARD, M.D.,

ALBANY, N. Y.

Col. J., American, married, 55 years of age, of healthy parentage and relations on both sides, has never had any serious illness, nor met with any injury ; has always been of perfectly correct habits in every respect ; smokes moderately and drinks an occasional glass of wine.

Eighteen years ago he had an attack of severe pain in the left side of the abdomen, the paroxysm lasting about six hours, and attributed by his attending physician to the passage of a renal calculus. With this exception, he does not remember to have had a pain or an ache in his life until his present illness.

About ten years ago he began to have hemorrhoids, which, about six years ago, commenced to bleed and annoy him ; but he could, and always did, return them when they protruded. This summer (1880) they had to be returned a dozen times a day, and he then placed himself under the care of a physician in Bridgeport, Conn., and in the course of five or six weeks was cured by what he calls a "process of absorption." He has since been entirely free from all annoyance from this source.

In Sept., 1878, when in Geneva, Switzerland, he passed, at one single urination, dark-colored urine which he considered to be bloody. The urine was clear the next time he emptied his bladder, and so remained until the second week in November, when, at Nice, he passed a larger amount of blood than at Geneva. The

next urination was again clear, and nothing further was noticed until, in going through the Pitti Palace, in Florence, in December, he experienced a sharp, burning pain in the end of the penis, which persisted for a day or two. He consulted Dr. Young, who said that the difficulty was due to the passage of uric acid crystals, and gave him alkaline treatment with *the result above stated*.

In Feb., 1879, at Rome, the urine remained dark for about a week, and deposited a mahogany-colored sediment. He was relieved by capsules of copaiba. In Leipzig, the first week in April of the same year, after a long carriage-ride over the pavements, he passed a larger amount of blood than ever before, but at no time suffered any pain while passing the blood. In Paris, the second week in April, the urine again became bloody, and so continued for about three weeks.

On April 28, 1879, he was carefully examined by Dr. Reliquet, who made the following diagnosis:—"Hémorrhôides. Congestion prostatique consécutive. De là le sang évacué spontanément avec l'urine." Dr. R., after careful examination, definitely excluded stone. On the theory of the diagnosis he was ordered to take Hunyadi Janos water every morning before breakfast, and a large rectal injection soon afterward, and sulphate and bicarbonate of soda three times daily. Thus the bowels were kept quite free and the urine alkaline; but on June 3d the urine again became bloody, and so continued at pretty short intervals until Sept. 15th. Then it gradually diminished and the urine was only bloody for a day or two at a time at intervals of about a month, and has so continued ever since. A long railroad ride would always bring it on.

Up to July 10, 1880, he had never had any pain or soreness connected with urination, except as above stated at Florence. At this time he began to have, at intervals, scalding pain the whole length of the urethra, accompanied by a desire to pass urine, and more or less straining. Urination was followed by an uncomfortable aching, which lasted five to ten minutes. Belladonna suppositories afforded great relief. This condition of pain and aching persisted and increased in severity up to the time that I first saw him, Oct. 7, 1880, when he was suffering severely and almost constantly.

In December, 1879, he first noticed an increased frequency in urination, and this became more and more of an annoyance until the latter part of August, 1880, he was passing water about every fifteen minutes during the day, and was obliged to get up at about the same intervals during the night, though toward morning he

might sleep an hour and a half or two hours. About the middle of September he began to pass urine involuntarily and during sleep unconsciously.

Oct. 7, 1880.—Saw him late in the evening for the first time professionally, and found him looking haggard and worn-out from loss of sleep and constant suffering. Urine dribbling constantly and tenesmus well marked. As he thought that morphine affected him unpleasantly we gave him codeia for the night.

Oct. 8th.—Had a much more comfortable night than usual, as far as the amount of sleep was concerned, but dribbled constantly and is still suffering as before. Put him upon half-drachm doses of lactic acid and washed out his bladder with a five-grain solution of the same.

The urine examined this morning contained quantities of pus; no considerable amount of blood; no casts; many crystals of triple phosphates; sp. gr. 1.008; reaction neutral; no sugar; a great deal of mucus; considerable epithelium; and after boiling, the addition of nitric acid, and standing six hours, the precipitated albumen occupied one third of the bulk of the urine used.

Oct. 11th.—The pus in the urine has largely decreased in quantity, and there is notably less mucus and epithelium, while its reaction is markedly alkaline.

Up to yesterday the patient kept passing gravel in small masses every hour or two, which on examination proved to be triple phosphates. The bladder has been washed out every day, though the process is more painful than with most patients. He says that his suffering is entirely changed in character and very much diminished, and the improved appearance of his face certainly corroborates his statement.

Oct. 12th.—Sounded him for stone with an entirely negative result. The passage of the steel instrument was effected without the least difficulty, and caused him very little pain; but on its withdrawal about an ounce of clear blood ran from the urethra and formed a firm clot on a cloth which received it.

Oct. 15th.—The constantly wet cloths in which the dribbling urine has been caught have made the head of the penis and the prepuce very sore, and the patient has therefore occupied the recumbent posture for the past twenty-four hours, and the urine has all been caught in a paper basin. We have thus been able for the first time to measure it accurately, and find that it amounts to 157 ounces, a larger quantity than we had supposed. There is very little pus now, but considerable ropy mucus the passing of which

causes almost as much pain as did that of the gravel. No great change in the urine except that to the naked eye the pus has almost disappeared, while the microscope still shows a few cells and some blood.

Oct. 16th.—In washing out the bladder to-day my attention was attracted by some small masses of tissue (apparently) floating in the fluid which now runs almost clear. These masses were examined by myself, and also submitted to Dr. Wm. Hailes, Prof. of Histology and Pathology in the Albany Medical College, who reports that "they are composed of papillæ, consisting simply of connective tissue, forming a support for capillary vessels which have their embryonic walls and terminate in loops in the ends of the papillæ." Neither he nor I could find any thing which was characteristic or even suspicious of malignity. Fig. 1 is a very accurate reproduction of the appearance of one of the portions examined, and all the rest were entirely similar.



FIG. 1.

Several detached papillomatous growths found floating in specimens of urine. They consist principally of connective tissue, forming a support for capillary vessels traversing the papillæ, and resemble benign papillomatous growths.

Rectal examination shows the mass to lie above the prostate, to be of considerable size and occupy a portion at least of the sides as well as the posterior wall of the bladder.

Oct. 18th.—The quantity of fluid passed from the bladder has diminished to 113 ounces in the past day, having been yesterday 125, and the day before 130. On the 16th the patient suffered so severely that he was obliged during the day and night to take three quarters of a grain of morphine. His stomach was disordered thereby, and his tongue, which has been thickly covered with a brownish fur all the time, was worse than ever. Pulse, which has ranged from 100 to 120, is to-day 105. The temperature has been at all times from 98° to 99°.

Careful physical examination of the thoracic and abdominal

cavities reveals nothing. To-day he has taken no morphine ; has been more comfortable than usual ; has suffered no pain except when passing the little masses of tissue above referred to, or the ropy mucus, and the latter has still further diminished.

Repeated examinations of the fragments of tissue passed show no change in their character, and the diagnosis, therefore, seems justified of simple papilloma of the bladder.

Oct. 21st.—Mr. J. has gradually and steadily improved ; eats and sleeps better ; has much less pain ; and the amount passed from his bladder has diminished to 87 ounces. The amount of albumen varies from one fourth to one third, and no casts can be found at any time. The lactic acid has been increased to drachm doses three times daily, and the washing out of the bladder has been discontinued, because the operation causes him so much distress at the time and for an hour or two afterward, while the disappearance of pus from the urine has done away with the necessity.

To-day Prof. Wm. H. Van Buren saw the case with me in consultation, verified the diagnosis of vesical tumor, advised against an operation, but recommended the use of *thuya occidentalis*, from which he had derived much benefit in similar cases. He also agreed with me that notwithstanding the presence of large quantities of albumen in the urine there was no good reason to suspect any disease of the kidneys.

Nov. 5th.—Since last date the patient has eaten and slept well, is cheerful and bright, and but for his local trouble would feel as well as ever.

The amount of fluid passed from his bladder has varied from 106 to 76 ounces ; the proportion of albumen from one to two fifths ; no casts at any time ; occasionally a little blood ; sometimes a few pus corpuscles, sometimes almost none ; there is always considerable ropy mucus entangling quantities of crystals of triple phosphates. The urea has been frequently estimated by the hypobromite method, and has varied from .50 to .59 of one per cent. No more villi have been found though they have been carefully searched for. The incontinence of urine is a great annoyance to him.

A faithful trial was given to the *thuya* but without any improvement in the condition of the urine, or any relief to the patient. In fact he suffered so much more than when taking the lactic acid that he asked to go back to the latter, and did so with decided relief.

He has now almost no vesical tenesmus, but suffers pretty sharply when passing the masses of semi-coagulated mucus with the crystals entangled in them, which seem to scrape and irritate the mucous membrane of the urethra.

Dec. 19th.—There has been little change in the patient's general condition since last date, but he is somewhat better. For a few days he has sat up in a chair, partially dressed, for about half an hour at a time, and eats very well. The constant dribbling of urine is a great annoyance to him whatever position he may assume. He has lost some flesh, but has gained in strength.

No change worth noting in the condition of the urine. Though the specific gravity remains low and the proportion of albumen very considerable, there are no casts at any time. The daily amount has averaged 85 ounces, with a minimum of 50 and a maximum of 108. Another trial has been made of the thuya with the same result as before, and the remedy which gives him the most comfort is, without doubt, the lactic acid. He suffers most when passing only a small amount of fluid; and at such times digitalis and acetate of potassa or other diuretics always afford relief. The infusion of digitalis was the most reliable, but could not be long employed at a time on account of its interference with the action of the stomach.

Jan. 28, 1881.—No change in the urine worth noting. Patient has had three hemorrhages from the bladder, one of which was quite copious. The blood coagulated in considerable amounts in the bladder, and the passing of the clots was the occasion of paroxysms of pain so severe as to demand the hypodermic injection of morphine, in half-grain doses at night, to procure any rest.

The family are exceedingly desirous that an operation should be done to remove the tumor, if not accompanied by too great risk. They are driven to this by the agony which the patient suffers, and are encouraged by reports which they have heard of success in cases which are narrated as being of a similar character. I therefore yesterday consulted Dr. Van Buren again as to the propriety of trying it. The arguments in its favor are that the patient has an excellent constitution and is in very fair general condition; the only fragments of the growth which we have been able to obtain have proved to be simple papillomatous villi, without any evidence of malignity; and the statistics of the removal of such growths are somewhat encouraging. Dr. Van Buren was opposed to the operation on the grounds that the growth was too large to be entirely removed, and that he was sat-

ified from the history of the case that the base of the growth was malignant. He thought it possible that at some future time the vesical orifice of the urethra might become occluded, and he would then advise the opening of the bladder, by the usual perineal operation, for the purpose of giving exit to the urine through a permanent fistula. If this had to be done he would then advise the removal, by tearing and scraping, of as much of the tumor as possible. Dr. Van Buren's advice was followed and no operation was attempted.

Feb. 24th.—The patient's sufferings have been relieved by morphine as demanded. He is worse in every way; the morphine interferes with his appetite and digestion and causes nausea and vomiting, though not as much as opiates given in any other way. Codeia and other drugs which do not interfere with the digestive apparatus exert no controlling influence over the pain. The morphine has been so objectionable that I gave to-night in its place fifteen grains of chloral hydrate with twenty of bromide of potassium.

Feb. 26th.—Mr. J. rested very well after taking the chloral night before last. Yesterday he suffered less pain than usual, and was notably dull and sleepy, though he could at all times be aroused and would then talk rationally and cheerfully. The drowsy condition was attributed to the chloral.

Last night, without chloral or any anodyne, he rested well, but this morning the drowsiness, which has considerably deepened, can not be attributed to any drug. An examination of the account kept of the amount of fluid passed from his bladder showed that it had diminished to an average of less than 55 oz. per day, and though there is no œdema of any portion of the body, there seems little doubt that the condition approaching coma in which he has lain for the past two days is due to uræmia. The administration of diuretics and sudorifics was therefore begun and a brisk cathartic given.

March 1st.—In consequence of the treatment the flow of urine has increased to an average of over 60 oz., and the bowels have moved freely. Of course the patient's condition is correspondingly better.

There seems no satisfactory way of accounting for the uræmia except on the supposition that the growth has occluded the orifices of the ureters and thus made pressure backward through them on the kidneys, impeding their action.

March 8th.—Up to the 5th inst. Mr. J. remained in very fair con-

dition, but suffering intense pain from the passage, at short intervals, of masses of coagulated mucus entangling large numbers of crystals. For the past few days the amounts passed have been as follows : March 2d, 47 oz. ; March 3d, 30 ; March 4th, 32 ; March 5th, 33 ; March 6th, 24 ; March 7th, 14 ; and to-day scarcely any—all this in spite of every effort made to effect an increase. Since the 5th he has been semi-comatose nearly all the time, aroused only when spoken to or by the paroxysms of pain. The suffering was so great that death, which came at 11 P.M., was almost a relief.

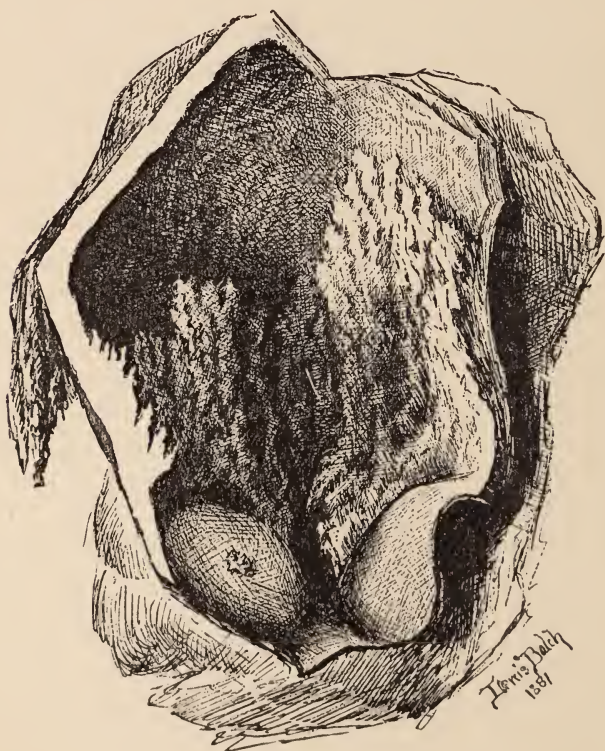


FIG. 2.

Gross appearance of bladder laid open by incision in anterior median line. Drawn by Dr. Lewis Balch.

Autopsy.—4 P.M., March 9th, 17 hours after death. Present, Drs. Vanderpoel, Sr. and Jr., Dr. A. Van Derveer, and Dr. J. S. Mosher.

General appearance, well-nourished. *Rigor mortis*, well-marked.

By request of the family the head and thorax were not opened.

Abdominal cavity opened by crucial incision. Omentum healthy and contained a fair amount of fat. Intestines healthy.

Bladder removed and opened by anterior vertical incision. Walls thickened; interlacing columns, formed by hypertrophied muscular bands, well marked; mucous membrane at the base covered with a soft cauliflower growth, extending all around the neck and up on each side—rather more on the left than on the right—so that one half, or perhaps a little more, of the internal surface of the bladder was covered with it. Orifices of the ureters entirely concealed by the growth and their lumen obscured by the neoplasm.

Both ureters were filled their whole length with urine and dilated to about half an inch in diameter, so as to easily admit the end of the little finger.

Left kidney: pelvis very much dilated, connective-tissue capsule quite adherent, cortical substance and pyramids apparently free from disease. Right kidney: apparently healthy, capsule not adherent, pelvis greatly dilated.

Not the least trace of disease was found in any of the other abdominal organs.

The heart was examined through an opening in the diaphragm and found normal.

Fig. ii, for the drawing of which I am indebted to my friend, Dr. Lewis Balch, gives a very correct idea of the appearance of the bladder and extent of the disease.

Remarks.—The most important point to determine in this case, in relation to advising an operation, was the exact character of the growth. There was no history of malignant disease in any individual of any branch of the family. The patient himself was entirely free from any cachexia; had, as far as careful examination could detect, no disease of any other organ; had always been perfectly healthy, with the exception of the supposed passage of a renal calculus eighteen years ago, which had never been repeated, and the hemorrhoids of which he had been entirely relieved when I first saw him. His bladder trouble dated back just two years to a passage of bloody urine, and that had been

repeated on numerous occasions. There had been entire absence of the pains said to be characteristic of malignant disease, indeed there had been no pain worth mentioning until the attack of acute cystitis during which I first saw him. Then portions of the mass obtained in washing out the bladder and subsequently in the urine proved, on careful microscopical examination, to consist of simple papillomatous villi, without a trace of any thing malignant. All this pointed to the existence of a benign growth which was the source of the hemorrhages, but which, could it be safely removed, would not, in all probability, return. The large size of the growth and its soft character as determined by rectal examination appeared to be the only sign pointing to any thing else. Yielding to Dr. Van Buren's clinical experience and declining an operation was proved by the autopsy to have been eminently judicious. Fig. i shows the exact appearance of three of the fragments obtained during life, and none of the others differed essentially from these. After the autopsy a portion of the tumor was sent to Dr. Hailes, who, with the aid of his freezing microtome, got sections through the whole length of these papillæ and the wall of the bladder from which they sprang, shown in fig. iii, and which could probably scarcely have been obtained in any other way. It is easily seen that the infiltration of malignant elements extends only a short distance up into the papillæ, and explains how the portions we obtained during life gave false evidence of the character of the growth as a whole. Fig. iv shows a portion of the infiltration still more highly magnified.

Another study of great interest in connection with this case was that of the urine. During the last four months and a half of Mr. J.'s life the average amount of fluid passed from his bladder each day was in round numbers 90 oz., the specific gravity of which stood very steadily at



FIG. 3.

Is a section through the mucous membrane of the bladder, showing two large dendritic papillary vegetations implanted upon a carcinomatous base, and resemble benign papillomata, except near the base.



FIG. 4.

Encephaloid carcinoma, showing well-marked alveoli, with the large epithelial cells of encephaloid disease. This field was taken from near the base of one of the papillæ shown in fig. iii.,

1010. After boiling, and the addition of nitric acid, the precipitated albumen occupied, after standing four hours, from one-third to three-fifths of the bulk of fluid used. Repeated and careful examination failed to show a single cast at any time. The urea varied from one-half to six-tenths of one per cent., which, considering the amount of fluid passed, was just about normal. There was not a single rational sign of any form of Bright's disease, and Dr. Van Buren's reply, when questioned on this point, was: "I will stake my professional reputation that the patient has no organic disease of the kidney."

I have no doubt that a large part of the fluid passed from the bladder was albuminous liquid exuded through the thin walls of the infinite number of villi constituting the exposed portion of this growth. The examination of a small portion of the fluid taken from the distended ureters at the autopsy supported this view, for it contained only a distinct trace of albumen and no more—less, in fact, than might have been expected considering the mode of death.

An interesting point in connection with the mechanical distention of the ureters and pelves of both kidneys, was that the same condition was found, on microscopical examination of the kidneys, to exist in the tubuli uriniferi, though the organs appeared to the naked eye to be healthy.

The persistent existence, then, of a large amount of albumen in the urine, without casts, without diminution of the total amount of urea excreted, and without rational signs of Bright's disease, might in other cases prove a valuable early diagnostic sign.

In the treatment of the cystitis, which was at first a very painful complication in this case, lactic acid gave more relief than any other drug, and its value has been attested in many other cases in my experience. For the suggestion

as to its use I am indebted to an article in the *Buffalo Medical and Surgical Journal*, for Feb., 1879, by Mr. Theodore Deecke.

The entire freedom from disease on the part of all the other organs of the body, and the fact that the recto-vesical septum was not invaded, were noteworthy.

I imagine that the mode of death by occlusion of the orifices of the ureters and the production of uræmic coma must be rare, for I do not find it recorded in the history of any case that has come under my notice. The probability of its having occurred was, in this instance, foreseen about a week before death.

The diagnosis from the *post-mortem* examination of the bladder, ureters, and kidneys would be carcinomatous papilloma of the mucous membrane and walls of the bladder, with hydronephrosis of both ureters and kidneys. There was also a marked dilatation of the straight and convoluted tubules in the medullary and cortical portions of the kidneys.

The soft papillomatous growth resembled cauliflower excrescences, and involved fully one half of the internal surface of the bladder, extending from the neck to the *bas fond* and sides.

This report, the microscopical examinations, and drawings were all kindly made by Dr. William Hailes, Jr.

Literature.—The treatises on general surgery all devote a few pages, more or less, to the consideration of tumors of the bladder. They all agree in dividing them into benign and malignant.

The names bestowed upon the varieties of the former class differ very considerably, and are founded rather on the results of microscopical examination after death, or in a few cases after removal, than on any possible clinical diagnosis. It is admitted on all hands that the growth

variously known as papilloma, villous papilloma, villous tumor, and vascular tumor, is the most common, and that simple fibroids, mucous polypi, and fibro-myomata occasionally occur.

Of the malignant growths encephaloid, scirrhus, and epithelioma are found, though authors do not agree as to their relative frequency.

As to the value of microscopical examination of portions of a tumor of the bladder obtained in the urine during life authors differ. Sir Henry Thompson, in his *Clinical Lectures*,¹ is cautious, basing his lack of confidence on the fact that there is no characteristic "cancer-cell," so-called. He speaks, however, of having "detected under the microscope the peculiar structure which the processes of the villous tumor present to the eye."

Harrison² says "the microscope is most valuable in detecting small portions of genuine villous growth," and the majority of authors speak as if finding such villi, free from any evidence of malignancy, would settle the diagnosis. In the case now reported a number of such villi were found, and yet the autopsy showed that any effort to remove the tumor by operation could have resulted in no good, in consequence of its nature.

Harrison says, at p. 361, that "tumors presenting some of the appearances of these villous growths, but of a malignant nature, are occasionally met with." But the distinguishing points which he mentions, such as induration, tendency to involve neighboring organs, implication of glands, and cachexia, were all absent in this case.

Gross³ says that papillary fibroma is frequently confounded with villous carcinoma, but we do not find that he

¹ London, A. & A. Churchill, 1876, pp. 351.

² *Surgical Disorders of the Urinary Organs*, Wood & Co., 1881, p. 358.

³ *Diseases, Injuries, and Malformations of the Urinary Bladder*, Henry C. Lea, 1876, p. 136.

any where mentions the possibility of what occurred in this case—that the base of the tumor proved to be a carcinomatous mass, originating in the bladder, not extending to any other organ, and covered all over with a growth of benign villi.

Gant¹ says nothing on this point.

Mr. Coulson,² in his admirably complete book, says “there can be no doubt that malignant growths occur, springing from mucous membranes, and having their surface covered with shaggy projections.” Mr. T. Holmes³ says, in two lines, “a cancerous tumor may also sometimes be covered by villous processes of mucous membrane.”

The lesson to be learned from this is that Sir Henry Thompson’s caution to his students is eminently sound: “Most valuable as is the microscope in this great class of maladies, ranking next and very near to the sound itself, never let it obscure for you those broad features of the case which are to be determined by the unassisted eye and touch.”

On the subject of the persistent albuminuria which was present in this case the general feeling of the profession is well laid down by Dickinson.⁴ He says that albuminuria may be artificially produced by the ingestion of a large quantity of highly albuminous food; perhaps by a rapid absorption of a large amount of serous fluid from the pleura, and some exceptional forms of hepatic disease. “With this exception, it may be stated, as a rule, that when the urine contains albumen the kidneys are abnormal either in circulation or in structure.”

Yet here is a case in which we think that a great and per-

¹ Frederick James Gant, *Diseases of the Bladder*, Lindsay & Blakiston, 1876.

² Walter J. Coulson, *Diseases of the Bladder and Prostate Gland*, Wood & Co., 1881, p. 116.

³ *Treatise on Surgery*, Henry C. Lea, 1876, p. 770.

⁴ *Treatise on Albuminuria*, Wood & Co., 1881, p. 11.

sistent albuminuria did not depend upon any form of kidney trouble.

The only reference we have been able to find to this point is in Mr. Coulson's book.¹ He quotes Dr. Ultzmann as pointing out in an article, *Ueber Hæmaturie*, in the *Wiener Klinik*, May, 1878, the fact that "in cases of villous tumor the urine always contains more albumen than corresponds to the blood or pus in the sediment." He accounts for it in the same way that we have in this case, though Mr. Coulson's book and this reference came under our notice long after the first part of this article was written. Dr. Ultzmann further points out the fact that fibrine sometimes appears in the urine under the same circumstances and from the same cause.

Gross mentions the possibility of death from uræmia from occlusion of the orifices of the ureters in the pelvis of the kidney. But we have not happened to notice the occlusion of the ureters at their entrance into the bladder.

The following histories of cases, more or less similar, related in medical journals, have come under my notice :

- | | | | |
|-----|--|-------|--------------------|
| 1. | <i>American Journal Med. Sci.</i> , vol. 16, | 1835, | page 522. |
| 2. | " | " | " 7, 1844, " 122. |
| 3. | " | " | Oct., 1874, " 561. |
| 4. | " | " | Oct., 1879, " 579. |
| 5. | " | " | July, 1880, " 233. |
| 6. | <i>Dublin Journal Med. Sci.</i> , vol. 16, | | " 333. |
| 7. | <i>Medico-Chir. Review</i> , | " 19, | 1831, " 453. |
| 8. | <i>London Lancet</i> | " 1, | 1849, " 43. |
| 9. | " | " 1, | 1850, " 188. |
| 10. | " | " 1, | 1854, " 212. |
| 11. | " | " 2, | 1880, " 978. |
| 12. | <i>British Med. Journal</i> , | " 1, | 1879, " 854. |
| 13. | <i>Med. Times and Gazette</i> , | " 1, | 1879, " 710, |
| 14. | <i>N. Y. Med. Record</i> , | " 8, | 1873, " 342. |
| 15. | " | " 14, | 1878, " 395. |
| 16. | " | " 16, | 1879, " 82. |
| 17. | <i>N. Y. Med. Journal</i> , | " 20, | 1874, " 62. |
| 18. | " | " 21, | 1875, " 503. |

¹ *Op. cit.*, p. 188.

19. *N. Y. Med. Journal*, vol. 23, 1876, page 299.
20. " " 27, 1878, " 166.
21. " " 28, 1878, " 629.
22. *Archives Générales de Méd.*, " 1, 1876, " 489.
23. " " 1, 1877, " 233.
24. *Medical News and Abstract*, 1881, " 854.

THE ACTION OF VERATRIA ON THE VENTRICLE OF THE FROG'S HEART.

By SYDNEY RINGER, M. D.,

PROFESSOR OF MEDICINE AT UNIVERSITY COLLEGE, LONDON.

IN these experiments I used Roy's tonometer. In most cases I employed the entire ventricle; in some only the lower two thirds, the portion free from nervous ganglia.

I used dried bullock's blood dissolved in water and diluted with saline in the proportion of one part blood to two saline.

In some experiments the ventricle beat spontaneously; in others contractions occurred only on the application of an induction shock.

A number or the sign + is given in the charts, to indicate the application of an induction shock. The number indicates the place at which the coil stood. The series of rises under either a number or + are due to a single induction shock.

The traces read from left to right.

Since temperature modifies the action of veratria on the ventricle of the frog's heart, I shall describe, first, my results with a high temperature, between 80° and 90° F.; then with a moderate temperature, between 60° and 70° F.; and, lastly, with a low temperature of about 40° F.

Effect of veratria with a high temperature.

The effect of veratria is in many respects best manifested when the heart does not beat spontaneously, but only on excitation, as shown in trace i, taken when the temperature of the room was close on 90° F.

Veratria first prolongs the duration of the systole, so that the summit of the amplitude becomes rounded; and this is still better seen in traces taken in a cooler room, or when the blood is cooled by ice.

Next, each excitation produces two contractions, but the interval is so short that the diastole is incomplete and the two traces become partially blended. In many instances, after the first contraction, the heart bulges at some point, and the diastole does not affect the whole ventricle; there is, indeed, incoördinate action of the ventricle. Each excitation soon induces a still more prolonged effect, and we get three, four, or more contractions produced in the manner just now described. Some portion only of the ventricle dilates, sometimes a small portion, sometimes a larger, and then re-contracts; and the degree of the trace-fall depends on the size of the bulging. In a few minutes each excitation produces a prolonged contraction, or series of contractions, lasting from 30 to 120 seconds; then after the excitation the ventricle suddenly contracts and the trace suddenly rises, then falls, sometimes considerably, at other times slightly, and the trace shows the ventricle is beating very incoördinately; but as the effect of the excitation persists this incoördination becomes much less, and in some cases even disappears. After a larger dose the trace suddenly rises, then falls slightly, and remains at nearly the same height as a straight line for 30 or 40 seconds; then the line becomes wavy. Though the line appears straight, a lens will often show that it is wavy, but in some cases the

line is quite straight. After it has become wavy for a variable time it slowly falls toward its original position on the base line. When the trace forms a straight line or the undulations are very small a considerable movement is visible in the ventricle, consisting of rapid, small, peristaltic waves of dilatation and contraction, running generally from base to apex.

With a larger dose (trace i, *C*), after remaining at much the same height for many seconds the trace slowly falls toward the base line, giving a trace much like the fall in a tetanized skeletal muscle.

The duration of the effect depends on the interval between the excitations. If, as soon as the effect of one excitation is over another excitation is applied, the duration of the effect of the second excitation is much diminished. In fact, the duration of the effect is in proportion to the duration of the diastolic pause. (Trace i, *B*.)

The irregularities in the trace are best studied by examining the ventricle. One sees extreme incoördination, especially as regards dilatation. A portion dilates when the rest remains contracted, hence pouching occurs, and some pouchings contract before others.

In these and other traces that I have taken it is noticeable that although the form of irregularity varies in different experiments, yet in each experiment the irregularity observes much the same type (see traces ii and iii).

At the end of a prolonged effect of an excitation the veratria irregularity often becomes less, and a ventricle at first very incoördinate at last beats nearly or quite coördinately (see trace i, *B*).

The sustained contraction, much longer after a large dose, which occurs for a brief interval after each contraction, is not due, as some hold, to an altered physical condition, lessening the elasticity of the ventricle, but

is due to muscular contraction—is, in fact, a prolonged spasm.

1. Either spontaneously or after an excitation the ventricle at first remains contracted for a variable time, then very incoördinate contractions ensue, and at last the contractions become quite coördinate (see traces i, iii, iv), and the ventricle stops in diastole. This series of events could not be explained were the elasticity of the ventricle alone affected.

2. In some cases after a contraction the ventricle relaxes greatly, but not completely, and remains in this semi-relaxed condition, and a strong vermicular action sets in; after a time the ventricle suddenly and completely relaxes with cessation of the vermicular action. This course of events cannot be explained on the supposition that veratria simply lessens elasticity (see trace vii, *F* and *G*).

3. Pouching, now of one part and next of another, cannot be explained on the supposition that the changes in the ventricle are due to lessened elasticity.

We see, then, that veratria induces, first, a change in the ventricle, so that each contraction is prolonged, and the amplitude of the trace is broader and its summit rounder; next, a single excitation, instead of one response, induces two contractions (see trace i, *A*. iii, *B*.); in other words, each excitation produces an increased expenditure of force.

As the effect of veratria progresses (more speedily after a large dose), each excitation produces a prolonged series of contractions, lasting thirty or more seconds, and the ventricular action becomes extremely incoördinate. Veratria, then, increases not only the expenditure of force set free by each excitation, but it lessens or destroys the conditions which make the action of the ventricle coördinate. These effects veratria produces by its direct action on the muscu-

lar tissue, since they were obtained with the lower two thirds of the ventricle, a part free from nervous ganglia.

Temperature strikingly influences the action of veratria on the ventricle. A small dose greatly affects the ventricle at a moderate or high temperature, but at a low temperature produces no effect; and at a low temperature a large dose, whilst greatly increasing the duration of the contraction (increased expenditure of force), only slightly incoördinates the action. It is probable, therefore, that the temperature of fever modifies the influence of remedies, and that veratria affects the heart more powerfully in fever than in health.

The incoördinate action is produced mainly, if not exclusively, by incoördinate dilatation; one portion dilates and forms a pouch, whilst the rest remains contracted. After each excitation there is, first, a coördinate contraction, but then some portions *remain contracted longer than others*, so that irregular dilatation ensues.

Veratria, therefore, affects the heart much as it affects the skeletal muscles. With a skeletal muscle veratria enormously prolongs the contraction and yet longer its relaxation. When the relaxation is complete well-marked fibrillary contractions set in, lasting many seconds, but produce no shortening of the muscles. These effects are not due to the influence of veratria on the nervous system, for they occur, I find, when the motor nerves are completely paralyzed by curare. With the skeletal muscles, then, veratria produces an increased expenditure of force, with incoördinate action of the fibrillæ.

In the muscular tissue (as, indeed, in nervous and secretory tissues) we have not only the fuel whose combustion supplies the force to the tissue, but we have, besides, a mechanism to convert the liberated force into muscular motion, nervous action, or secretion. There must also be a mechan-

ism to regulate the amount of energy developed, and to co-ordinate the action of muscular and nervous tissues. A poison may affect the combustibility of the fuel, or the mechanisms, or both, and so paralyze a structure. Veratria, I venture to suggest, disorders the machinery, and so causes at first an increased expenditure of energy, then incoördinate action, and at last complete paralysis.

The action of veratria on muscle is in many respects comparable with the effect of strychnia on the spinal cord. Instead of a coördinated response of short duration, an excitation calls forth from a strychnized cord a prolonged and incoördinated action. The slightest excitation spreads throughout the cord and causes a general evolution of force, lasting a considerable time.

Like veratria on muscle, strychnia weakens or destroys those conditions in the cord which determine the amount of force evoked by an excitation; hence the prolonged effect of an excitation on the strychnized cord. At the same time, strychnia destroys the mechanism which coördinates action, so that the response is incoördinate.

Continued strong faradization affects the heart much like veratria. Under the influence of a strong interrupted current the ventricle becomes contracted, then small bulgings occur, sometimes at one portion and then at another, and these bulgings immediately contract again. After a time the bulgings increase in size, and then both systole and diastole become most irregular.

If a poison can cause incoördinate action by its direct action on the muscular tissue, and independently of its action on the nervous tissue, it is probable that disease of the muscular tissue may also cause muscular incoördination. The fibrillary twitchings in progressive muscular atrophy, which are so similar to the fibrillary movements after poisoning by veratria, are probably due to changes affecting

the muscular machinery; also some cases of irregular action of the heart are probably due to disease of the muscular substance, and not of the cardiac nervous apparatus.

Further, the lower two thirds of the ventricle, the portion free from nerves, contracts coördinately, spontaneously, or when excited; and this implies a coördinating mechanism which disease may destroy, and so produce incoördination.

Atropia in some respects antagonizes the action of veratria, for atropia stops or greatly lessens the irregularities, though the heart's action becomes much weaker.

Traces viii, viii (a), and ix, show the antagonizing effect of atropia.

Explanation of Plates.

Effect of veratria with a high temperature.

Trace i.—With the whole ventricle, which did not beat spontaneously. *A*, Effect of 5 minims of 1-in-500 solution of veratria added at the point indicated by the arrow. *B*, Nine minutes later. *C*, After an additional 20 minims of the solution.

Trace ii.—With the whole ventricle, which did not beat spontaneously. Well shows the effect of a single excitation, after a larger dose of poison. Here there are no undulations in the early part of trace following the excitation, even with the aid of a lens. After 40 minims of 1-in-500 solution.

Effect of veratria with a temperature between 60° and 70° Fah.

Trace iii was obtained with the lower two thirds of the ventricle, a portion of the heart free from nervous ganglia. It is a good instance of the effect of veratria when the temperature of the blood is between 60° and 70°. As the effects from this segment of the ventricle are the same as those which occur when the entire ventricle is used, it is evident that the veratria manifests its action on the muscular tissue.

As with a higher temperature, so veratria with a moderate temperature first prolongs each contraction. Then each excitation induces two contractions; but as the second contraction does not begin till the completion of the first, they remain dis-

tinct, instead of being blended, as happens when the temperature is higher. Next the trace becomes affected, as with higher temperatures, and need not be more fully described here. This portion of the ventricle did not beat spontaneously. *A*, Before veratria. *B*, After 5 minims of 1-in-500 solution. The + indicates the excitation. It is seen that each excitation induces two contractions. *C*, After a larger dose. *D*, After a still larger dose.

Trace iv.—With the whole ventricle. Shows the gradual development of the veratria effect. *A*, Before veratria. *B*, Five minims of 1-in-500 solution added at the point indicated by the arrow. *C*, Eighteen minutes after veratria. *D*, About forty minutes after veratria.

Trace v.—Shows the effect produced by a large dose at a moderate or high temperature. Each rise becomes broader, then coalesces with its fellow; that is, one contraction begins before the previous one ends, and at last forms nearly a straight line raised high above the base line.

This trace was taken with the whole ventricle. At the arrow ten minims of 1-per-cent. solution of veratria was added.

I now give the effect of veratria, with blood cooled, by a freezing mixture, down to 8° or 10° Fah.

Trace vi.—With the whole ventricle. *A*, Before veratria and after the blood was cooled to about 8° to 10° Fah. *B*, About nine minutes after 15 minims of 1-per-cent. solution of veratria. *C*, About eighteen minutes after veratria. *D*, Later effect.

Sometimes even at a low temperature veratria in large doses incoördinates the ventricle, but in a far less degree than when the temperature is high. This is exemplified in trace vii, which also shows the alternating effect first of cold, then of heat, and then again of cold.

Trace vii.—With the whole ventricle. *A*, Temperature of blood 63° F. *B*, Temperature of blood 45° to 46° F. *C*, Two minutes after 15 minims of 1-per-cent. veratria solution. Temperature of blood 43° F. At first there occurred decided irregularity. *D*, About eight minutes after veratria. *E*, eighteen minutes after veratria. Temperature of blood 40° F. I then removed the ice-and-salt mixture. *F*, Thirty-six minutes after veratria. Temperature of blood 64° F. Here after the rise a decided fall took place, and for some time, eight to ten seconds, the trace remained at the same point, and then suddenly fell. Whilst it remained horizontal, and before the final sudden fall, very distinct frequent peristaltic action occurred, generally proceed-

ing from base to apex. After the final fall this ceased. *G*, forty-five minutes after veratria, temperature of blood 81° F. Here the tracing is the same as in the last, except that the primary fall was greater and the final fall very slight. I then raised the temperature of the blood to 86° F. and the heart's action grew very weak and infrequent. *H*, fifty-four minutes after veratria, blood 86° F. I then again applied the freezing mixture. *I*, Sixty-three minutes after veratria, blood 59° F. Here, again, after the primary fall and before the final fall, well-marked frequent peristalsis occurred. *K*, Seventy-four minutes after veratria, blood 41° F.

Tracings showing the antagonizing effect of atropia.

Trace viii.—With the whole ventricle, temperature of blood about 90° Fah. *A*, Effect of 5 minims of 1-in-500 veratria solution. At the point indicated by the arrow 10 minims of 1-per-cent. solution of sulphate of atropia were added. *B*, At the arrow other 10 minims of 1-per-cent. atropia solution added.

Trace viii (*a*). Temperature between 60° and 70° Fah. *A*, Before veratria. *B*, Effect of 5 and 10 minims of 1-in-500 solution of veratria. *C*, Shows the effect of atropia; at the arrow 20 minims of 1-per-cent. solution of sulphate of atropia added. *D*, Ten minutes later.

Trace ix.—With the whole ventricle, temperature of blood 66° Fah. *A*, Before veratria. *B*, After 30 minims of 1-in-500 solution of veratria. The notch in this trace was due to a broad peristaltic wave of dilatation and contraction, and shows that when this is large enough it affects the trace; at other times, when smaller, as previous traces show, the trace is unaffected. *C*, After 70 minims. *D*, After 10 minims of 1-per-cent. solution of sulphate of atropia. *E*, About a minute later.

DRAINAGE IN THE REMOVAL OF SUB-MUCOUS FIBROIDS.

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THE importance of thorough systems of drainage, both from hygienic and surgical standpoints, has received much consideration of late, and has given an increased stimulus alike to the sanitary engineer and the surgeon, in furnishing efficient means for the removal of deleterious material. The surgeon sees the most immediately serious results from delay in the establishment of good drainage. He therefore arranges for the escape of effete matter even in advance of its formation, and is greatly concerned at any interference with the thorough working of the means thus provided.

Although the adoption of Lister's antiseptic principles has, to a certain extent, diminished the necessities of drainage, either by lessening the amount of obnoxious fluids, or by absolutely preventing processes resulting in their formation, yet the careful surgeon must always prepare a way of escape for such objectionable material, let its cause be what it will. The amount of tissue destroyed and left to be taken care of in the reparative process may be great; some defect may exist in the perfect carrying out of the antiseptic treatment; and even when there is no defect, the difficulties in its way are often serious in those parts

having a natural outlet which must, in a measure, be kept patent. Whether these causes are present at the time of the primary operation, or arise subsequently to it, the surgeon knows that they are best dealt with at the time of the operation, and that his patient may thus oftentimes be spared the ordeal of a second surgical interference.

The necessity of providing drainage in the removal of sub-mucous fibroids was first presented to me in a case reported by my honored instructor, Dr. J. Marion Sims, in his valuable article on "Intra-Uterine Fibroids," published in the *New York Medical Journal* for April, 1874, being case No. 6 of those reported. The case was indeed most grave, the patient being blanched and anæmic to the last degree; while the tumor, fibro-cystic in character, reached above the umbilicus. The os uteri was fully dilated; and the vagina was filled with the growth, which, in that part, was nearly sloughing. It was the first operation of the kind I had ever witnessed; and I can never forget the boldness and brilliancy of each step of its performance, nor the profound respect and admiration I felt for the operator.

It was my fortune to have the subsequent treatment of the case, which consisted locally of the frequent irrigation of the cavity of the uterus with carbolized water for several days after the uterine and vaginal tampons were removed. Suddenly on the nineteenth day, the intra-uterine injections having been discontinued, the patient had a chill followed by intense fever, the temperature being 106° and the pulse 160. It was then found that the uterine structure was so softened as to allow a flexion of the organ upon itself at the os internum, thereby occasioning an obstruction to the free flow of pus from the uterine cavity. I succeeded in adjusting a block-tin tube, three inches in length, in the cervical and uterine cavities, which was left in place as long as any discharge continued. It acted most satisfactorily;

the patient had no more chills or other indications of septic absorption, and made a good recovery. The great benefit derived from the drainage tube in this case determined me to try it in necessary instances subsequently, substituting a hard-rubber tube for that of block tin, and making its application at the time of the operation instead of waiting until dangerous symptoms presented themselves. I now wish to present seven cases in illustration of the advantages to be derived from this plan of treatment, as well as some facts regarding the form and adjustment of the tube, taught me by my experience.

CASE I.—Mrs. H., forty-one years of age, had been married five years, and had had one child then four years old, and no abortions. The menstruation, which, up to the birth of her child, had been perfectly regular and normal, at that time began to increase both in amount and duration, until the flow would continue active for nine or ten days and oftentimes drag on several days longer, causing exceeding exhaustion. During these days the patient was obliged to remain in bed, using the various ordinary means to control the hemorrhage. For some months she had taken large doses of Metcalf's fluid ext. of ergot, amounting more latterly to nearly $\frac{1}{2}$ ounce three or four times daily, but had obtained very little control of the hemorrhage, and little if any contractile effect upon the uterus. The depth of the uterine cavity was fully five inches. Among many minor troublesome symptoms complained of, was frequent micturition.

For the purpose of exploring the interior of the uterus and removing, if possible, the sub-mucous fibroid which had previously been diagnosticated, a sponge tent was inserted into the uterine canal March 28, 1878. After sixteen hours this was removed, and seven laminaria tents of large size substituted. Seven hours were allowed for dilatation by these last tents, and, ether having been administered, assisted by Dr. J. P. Reynolds and Drs. E. G. Cutler and C. M. Green, I was able to remove the fibroid, which was of the size of a large goose egg and attached over fully one third of its surface. The enucleation was accomplished by keeping up a constant traction with the volsellum forceps, cutting through the capsule at the lowest point of attachment, and separa-



FIG. 1.

ting the growth from the uterus with the finger. The stronger bands of attachment were cut with the scissors, the points of which were guided and guarded by the finger. The growth was entirely within the body of the uterus, not having pressed sufficiently upon the internal os to occasion the slightest dilatation or softening there. As soon as the tumor was removed and the portions of capsule which were free had been trimmed off, a hard-rubber drainage tube, $2\frac{1}{4}$ inches long, with an internal diameter of $\frac{3}{4}$ of an inch, was inserted in the canal of the uterus, its inner end reaching through the internal os, and its external end coming flush with the external os, where it was retained in its position by two silver sutures passed through the anterior and posterior lips of the cervix respectively, care being taken not to twist them too tightly lest they might cut themselves out of the tissues. Two small holes had previously been made in the tube for the reception of the sutures (fig. 1). The cavity of the uterus was then thoroughly syringed with hot water, and Churchill's tinct. of iodine applied to it. A tent of styptic-iron cotton was placed in the drainage tube, and the vagina was tamponed with ordinary cotton. Not more than one ounce of blood was lost during the operation. With the exception of some irritation of the throat occasioned by the ether, the patient felt comparatively comfortable after the operation.

The tampon was removed March 30th, and the uterus thoroughly washed out with carbolized water three or four times daily for a week afterward, by placing the long nozzle of the fountain syringe directly into the drainage tube. On the third and fourth days the temperature reached 101° and the pulse 100, the highest points noted. By the fifth day they both became normal and afterward remained so. From the third to the sixth days there was more or less muco-purulent discharge, with occasional shreds not only washed away with the injections but also discharged upon the napkins between times. By the eighth day all this had been entirely absent for two days, and the pulse and temperature had been normal for three days; the drainage tube was therefore then removed and vaginal injections substituted. On the fourteenth day, the depth of the uterine cavity was three

inches. The patient was that day allowed to sit up, and the next day to be about her room. The recovery was uninterrupted. A year afterward the uterus was but two and three quarter inches in depth, and the patient reported the menstruation as having been perfectly normal since the operation.

CASE 2.—Mrs. S. was admitted to the Free Hospital for Women, March 17, 1878. She gave every evidence of some terrible drain upon her. I have seldom seen a person in so bloodless a condition. She was 32 years of age, and had been married twice, having had two children by the first husband, but no abortions. The menstruation, which began when she was 14 years of age, was regular and normal up to the death of her first husband, seven years before. At that time she noticed an irregularity in its recurrence, being a shortening of the intermenstrual period. The flow was also very pale in color. Three months before her admission to the hospital she had profuse menorrhagia, since which time she had kept her bed. Previous to this hemorrhage, there had been no undue flowing, but rather a diminished amount, sufficient sometimes to soil only one napkin. She had noticed a swelling in the hypogastrium for seven months, which had not, however, perceptibly enlarged during this time. She had complained of considerable leucorrhœal discharge before taking to her bed. Her appetite and digestion were good. A physical examination was made, and the case diagnosticated as one of sub-mucous fibroid of the uterus. Being in hospital, where any hemorrhage could be controlled, should it recur, it was thought best to bend all efforts to gaining a certain amount of strength preparatory to the operation for the removal of the growth. She was therefore ordered a generous diet, cod-liver oil, and iron. This treatment was persisted in for two months, at the end of which time she began to show some color in her cheeks and hands, and to be about the room. During these eight weeks there were two slight attacks of flowing, which were readily controlled by the vaginal tampon.

May 24th, the patient gave evidence of being not quite clear mentally; and the temperature, previously normal, rose to 102.5°. At the same time there appeared an offensive and purulent discharge from the vagina. There was every indication that the tumor had begun to slough. Its immediate removal was determined upon, and one large sponge tent was introduced. Sixteen grains of quinine were also given. In twelve hours the sponge tent was changed for six laminaria tents, by which time the patient was quite delirious.

May 25th. The patient was etherized by Dr. C. M. Green, the house-surgeon, and, the tents being removed, full dilatation was found to exist. The tumor was firmly adherent to the left side and anterior part of the uterus, from the fundus to the os internum, and was sloughing superficially. The removal was similar to that in Case 1, except that, as the tumor was much larger in size, it was necessary to remove it in two pieces. Several large shreds were also cut away. There was almost no loss of blood. The drainage tube and vaginal tampon were adjusted as in the first case. On account of great weakness of the pulse, subcutaneous injections of brandy, and also quinine, were given during the operation, and for several hours following it. Within thirteen hours after the removal of the tumor, the temperature had fallen to 98.9° , while the pulse was 96 and fairly strong. The next day, however, the temperature again began to rise; the vaginal tampon was removed, and vaginal injections of carbolyzed water instituted and continued each four hours.

May 27th. Large flakes of degenerated material and shreds were washed away, the nozzle of the syringe being placed in the drainage tube. By June 3rd these had entirely disappeared, the water returning perfectly clear and inodorous. The tube was removed on the eighth day after its adjustment, and there was no perceptible discharge subsequently. The temperature ranged most of the time between 99° and 100° , and the pulse between 90 and 100. It was not necessary to use any opiates, there being no special pain complained of. There was more or less low muttering delirium, beginning the day before the operation and continuing nearly to the time of her death, June 14th. Five days before death, she was unable to talk intelligently, and could not protrude the tongue; there was also diminished sensation and partial paralysis of motion in the right cheek, right arm, and right leg, as well as considerable œdema of the arm and leg. No reaction was obtained from the right pupil. There was incontinence of urine. Two days later, although the swelling of arm and leg had increased, yet in the arm the power of motion was partially recovered. The day before death there was a diminution of the swelling of the right leg, and the femoral vein was felt to be hard and cord-like. Throughout nearly the whole time after the operation, the patient was sleeping, being easily aroused for her nourishment, which she was always ready for, taking milk, beef tea, and brandy freely.

The autopsy, performed by Dr. A. T. Cabot 18 hours after

death, showed a hemorrhage between the dura and pia mater over the right middle lobe, partially clotted and partially fluid-blood. The posterior branches of both middle cerebral arteries were closed by emboli, the resulting thrombi extending into the smaller branches completely closing them. About one third of each middle cerebral lobe, being the parts nourished by the occluded vessels, were softened. The lungs were œdematous. The vessels below the Fallopian tube in the right broad ligament were filled with clots partially decolorized. This thrombosis of the vessels involved the iliac vein, which was occupied by a thrombus extending from the region of Poupart's ligament to the junction of the vena cava. The proximal portion of this clot was light-colored and friable; the distal portion, dark red and recent. There was also a recent thrombus in the femoral vein of the right side. The uterus was four inches long, and from three quarters to one inch in thickness. The mucous membrane was somewhat thickened and injected on the left side, with a slightly projecting eminence from the left side of the wall, suppurating on its surface. The vessels of the body were dilated and thickened.

This case well illustrates one of the great dangers to which these cases are subjected. Reduced by excessive hemorrhage, with the blood in the most favorable condition for the formation of a clot, and with the greatly increased activity in the circulation of the uterine blood-vessels, death may occur, as the autopsy proved in this case, from emboli plugging some of the cerebral arteries. This danger, together with that from septic absorption from a sloughing fibroid, must be cautiously considered, and full importance ascribed to each, when we decide to delay our operative interference. In the case just cited, the autopsy showed involution to have begun, and the healing process in the uterus progressing favorably; and the result might not have been different had the operation been done earlier. Yet I can but feel that the patient might have had a better chance had the tumor been removed months before. The next case, although not one of uterine fibroid, yet illustrates so well not only the importance of drainage, but also one of

the difficulties which may occur, necessitating a change in the form of the tube used, that I have introduced it into this series.

CASE 3.—*Retained fœtus and adherent placenta.* Mrs. B. was admitted to the Free Hospital for Women in the spring of 1878. She was 35 years of age, had been married 15 years, and had had two children, who were then 13 and 9 years old. There had been no abortions. There was nothing of special interest in her previous history. She was a farmer's wife, and had always enjoyed fair health. Menstruation appeared when she was 13 years of age, and had been regular and normal, with the exception that she had always suffered from dysmenorrhœa. The last menstruation had occurred in December, 1877. It was absent in January, and at the time for its recurrence in February she took cold, had a sick headache, and began to flow. This flow continued, with intervals of cessation, for six weeks, the patient being on the bed or sofa only part of the time, but losing much blood and suffering much from nausea. She did not think herself pregnant. The day following her admission to the hospital she was etherized; and, on removing the vaginal tampon placed by the house-surgeon, Dr. C. M. Green, at her own home, in order to control the hemorrhage and enable her to be moved to the hospital, a partially decomposed fœtus, apparently about three months developed, with a portion of the placenta, was found lying in the vagina. After their removal, the interior of the uterus was explored and about one half of the placenta was found firmly adherent to the left side of the uterine wall. This tissue was not soft and friable, easily breaking down under the finger, or removed by forceps; but in its feel very much resembled sole leather, and seemed so intimately connected with the uterus as to form a part of it. It was a difficult and tedious matter to remove it. Holding it steadily with the volsellum forceps, and guiding the scissors over the point of the left forefinger, this very tough tissue was cut from the uterine wall, the thickness of which was from time to time determined by bi-manual touch to assure myself that this organ was not cut through. An intra-uterine douche of hot carbolized water was then given, and ordered to be repeated each six hours, and the drainage tube adjusted as in the first and second cases. She was also ordered 4 grains of the aqueous ext. of ergot by the rectum *ter die*. The second day after the operation the patient had a slight

chill; and the temperature, which had previously been 101.4° , at once rose to 103.7° , and the pulse to 140. On seeing the patient, I learned that there had been no foul discharge or shreds returned with the washings, which had been given by inserting the end of the vaginal nozzle of the syringe into the end of the drainage tube. I attempted to pass a double catheter through the latter, and found the os internum closed over its inner ends. The force of the uterine contractions had evidently expelled the tube from the cavity of the uterus; but it was unable to slide from the cervical canal, as its outer end was stitched to the lips of the cervix, and the force of these contractile efforts had stretched out the cervix so that it contained the whole tube in its canal. (Fig. 2.) Under these circumstances the drainage tube could, of course, accomplish nothing. It was therefore removed, and a double silver catheter of large size passed into the interior of the uterine cavity, and made fast to the external parts by strips of adhesive plaster. There was at once discharged through it fully an ounce of purulent and highly offensive fluid. The intra-uterine injections were continued each two hours for two days, then each three hours, and then each four hours, until, by the eighth day after the operation, all discharge having ceased, the drainage tube was removed altogether. There was no after chill or elevation of temperature; and the patient was considered quite convalescent, when she suddenly, from some undue exposure, developed a slight attack of pneumonia, which kept her in the hospital about four weeks longer than would otherwise have been necessary. At the time of her discharge, which was between seven and eight weeks from the date of admission, the uterus had quite recovered its normal size. Within the past year I have heard of the safe delivery of this patient with her third child.

To prevent a like accident in future I had the hard-rubber drainage tube made longer by having its inner end bevelled after the shape of an ordinary Ferguson speculum. (Fig. 3.)

CASE 4.—Miss G. first consulted me January 29, 1878, complaining of dysmenorrhœa and leucorrhœa. She was 36 years of age, and a teacher. Her family history and her own general



FIG. 2.



FIG. 3.

health were good. Menstruation appeared at 15 years of age and occurred regularly afterward. At the time of my first seeing the patient, the flow continued seven days, and was excessive, amounting to twelve thoroughly saturated napkins during the first two days, besides the discharge of many large and solid clots. The pain was described as most severe, being mainly dull, heavy, and grinding in character, coming on several hours after the appearance of the flow, and lasting perhaps two days, or during the time of its greatest excess, and then subsiding to reappear for an hour or two daily for four or five days more. The leucorrhœal discharge was thick and stringy, like the white of an egg, but not sufficient in amount to require protection. There was no difficulty in walking, no headache, and no trouble of the bladder or bowels. An almost constant backache was, however, complained of. A physical examination showed the body of the uterus ante-flexed, with stenosis of the canal at the os internum which scarcely allowed the most delicate Emmet's probe to pass. A month later, a sponge tent was introduced, and a larger one substituted in twelve hours. Upon examination under ether six hours later still, it was found that the stricture had yielded but slightly to the tents, the touch, practised bimanually, conveying the impression of the existence of a small interstitial fibroid pressing on the os internum posteriorly. Two months after this examination, the cervix was divided bi-laterally, to the crown externally, and at the os internum to the depth of $\frac{1}{4}$ of an inch. A conical glass plug was placed in the canal and the vagina tamponed. The patient made a good recovery from this operation, and reported to me again in January, 1879. Examination showed the calibre of the canal sufficient to admit readily a Peaslee's sound. But the dysmenorrhœa and menorrhagia both existed still, not having been relieved in the least degree by the previous treatment. She was now referred to the Free Hospital for Women, where she remained for five months, faithfully carrying out a general tonic and rest treatment, attempts being made to control the hemorrhage at the menstrual periods by the tampon—ergot not having any effect in accomplishing it. During this time she improved greatly in flesh and general strength, but the dysmenorrhœa rather increased. During the summer she was in charge of Dr. G. W. Porter, of Providence, who removed with the curette some hypertrophied glandular structure of the interior of the uterus without any previous dilatation. This seemed in a measure to relieve the menorrhagia, although the dysmenorrhœa was much less affected by the

treatment. On her readmission to the hospital in October, 1879, the uterus was still anteflexed; the depth of the uterine cavity was three inches, and the calibre of the canal quite normal.

After three successive menstrual periods, an attempt at dilatation was made by means of tents, in order more thoroughly to explore the internal cavity. The first two times the tents were pushed below the os internum by the violent contractions of the uterus, even though the vagina was firmly packed to keep them in place. The third time, however, the precaution was taken to pass a silver suture directly through the laminaria tent, which was of large size, and also through the anterior and posterior lips of cervix. When this was removed, after twenty-four hours, it was found that the os internum was sufficiently dilated to force the forefinger through it. The touch then made the presence of a very small fibroid in the posterior uterine wall absolutely certain. It was so embedded that its inner surface projected but slightly into the uterine cavity. Its lower portion pressed against the internal os, and its size was that of an English walnut, somewhat flattened. Its removal was one of the most difficult operations I have ever performed. The os internum was not sufficiently open to admit the finger and at the same time allow the use of the necessary instruments for its removal. (Fig. 4.) Nor could it be further dilated on account of the situation of the fibroid, extending, as it did, somewhat around upon the side. Thomas' scoop was of no use, because it could not be introduced. By cutting into the tumor with scissors just within the os internum, I was soon able to detach enough of the growth from the uterus to allow me to seize it with a small pair of volsellum forceps; and, by continued traction and persistent efforts with scissors, finger, and at times one part of the handle of the scissors, I was at length able to remove the most of it, although it was the work of an hour and a quarter to accomplish it. The drainage tube, as modified, of a somewhat smaller circumference, was adjusted in the cavity of the uterus and stitched to the cervix externally; and the other vaginal dressings were applied as before. In the evening of the day of the operation, Feb. 23, 1880, the tem-

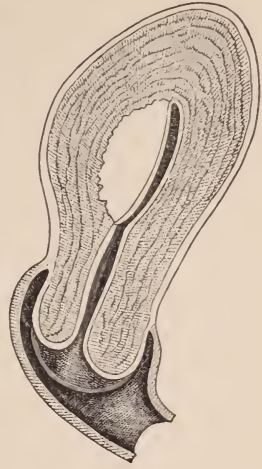


FIG. 4.

perature rose to 100° ; but the next morning it was normal, nor did it afterward rise above 99° . The pulse varied between 80 and 90 beats to the minute. The vaginal dressings were removed on the second day, and the intra-uterine injections continued after-

ward until the removal of the drainage tube on the tenth day, after all discharge had ceased. The patient was sitting up in two weeks from the date of the operation, and in two weeks more was discharged from the hospital cured. I have heard from her within two months, and there has been no severe dysmenorrhœa nor any abnormal amount of flow since the operation. In the prolonged care of this case, much credit is due my house-surgeons, Drs. F. W. Johnson and C. P. Strong.

CASE 5.—Miss H., æt. 36, consulted me May 8, 1880, on account of excessive menstruation, which had been gradually increasing for four years until she had begun to experience some feeling of debility from the continued drain. The time of the continuance of the flow had also lengthened from four days to a week and sometimes more. There had never been any dysmenorrhœa nor other symptoms that would indicate the nature of her trouble. She had always been very strong and well, never having to consider her own strength at all, whether for the continued tax of travelling and sight-seeing, or for the more tiresome duties of home and social life. Feeling assured herself that something must be wrong locally to account for the excessive flowing, she decided that it should be satisfactorily determined. For this purpose, ether was at once administered, and the body of the uterus found bi-manually



FIG. 5.

a little enlarged and anteflexed. By the passage of the probe, the uterine depth was found to be $3\frac{1}{4}$ inches. Dilatation and exploration of the interior were advised; but, on account of her family changing from their winter residence in the city to their

country home, it was postponed until July 21st. I was assisted by Drs. F. H. Davenport and J. W. Elliot. The canal had been previously dilated with the sponge, followed by laminaria tents; but I found that in this case, as in the last, the tents had been expelled from the uterine cavity, and were lying in the cervical canal.

Rather than expose my patient to the greatly increased danger of at once inserting another set of tents, or yet to the delay of waiting until after another menstrual period, I decided to attempt rapid dilatation with a set of hard-rubber plugs which I had long previously had made for the purpose (fig. 5), graded in size from $\frac{3}{8}$ to $\frac{3}{4}$ of an inch in diameter, and three inches in length. They are supplied with steel handles screwed into the rubber plugs. It was the work of an hour to dilate sufficiently to allow the forefinger to pass into the uterine cavity. This time might have been very much shortened had I been willing to use either uterotome or scissors in dividing the cervix externally, internally, or both; or even had I used one of the more forcible instruments with divulsive blades. But with the dilators described and figured, I was able to avoid making any unnecessary denuded surface to be subsequently exposed to septic influences, and, at the same time, to distribute the force equally, simulating as nearly as possible the natural forces in the process of dilatation for the passage of a fibroid from the cavity of the uterus. This part of my work accomplished, fully another hour was needed to enucleate and remove a sub-mucous fibroid of the size of a large olive from the posterior and left lateral walls, situated pretty low down, and attached throughout three fourths of its extent. The opening through which I worked was exceedingly contracted. The tumor was rather friable, and was removed in shreds. The modified drainage tube of the smaller size, as in Case 4, was adjusted, and the after-treatment carried out in a similar manner with the most gratifying results. There was, however, in this case, much more abdominal tenderness on account of the prolonged efforts at dilatation; and for three days the temperature ranged between 100° and 101.8° , not rising afterward above 99° . There was at no time any indication of inflammatory action, and the drainage tube was removed on the eighth day. I have heard from the patient several times during the past year, and the menstruation is in every way perfectly normal.

CASE 6.—Mrs. S. was referred to me by Dr. Bacon, of Brockton, July 28, 1880. She was 42 years of age; had been married

25 years ; and had borne four children, the eldest of whom was 24 years old, and the youngest 11. Patient had never aborted. About a year before I first saw her, after recovering from an attack of diphtheria, she first noticed that she had pain in turning in bed, and also discovered an enlargement above the pubes.

Menstruation began at 12 years of age, and had been perfectly regular and normal up to the birth of her last child. After this event, the time of its continuance lengthened a day or two, and the amount increased, first from 2 or 3 napkins to 4 or 6, and still further increased gradually, until during the past two years the duration of the flow had become from 7 to 9 days, and the amount from 20 to 25 saturated napkins, besides the discharge of many large clots. She had also had two severe hemorrhages, five months and one month respectively, previous to consulting me. Much exertion of any kind would start the flow. She had suffered from a constant leucorrhœal discharge for five months. She was also troubled with a very frequent desire to micturate. She had been unable to do any work (dressmaking) for several months. A physical examination showed the uterus to be strongly anteverted, and in size about that of the gravid uterus at the third month. The probe passed into the uterine cavity 5 inches. The diagnosis of sub-mucous fibroid was made, and its removal advised.

Aug. 19th. The menstruation having been controlled by ergot, and having ceased four days before, the canal likewise having been dilated with sponge and laminaria tents, the patient was etherized, and, assisted by Drs. Davenport, Bacon, and Elliot, I enucleated the tumor in a manner similar to that already described, except that the operation was much expedited by the use of Thomas' scoop. In size the tumor was equal to a large Florida orange. It was impossible to remove it as a whole ; and it was cut into several pieces before it was all taken away. It was attached over the whole extent of the anterior wall and fundus of the uterus ; and quite a large denuded surface was left after its removal. The drainage tube of the larger size was adjusted, as in the former cases, the uterus washed out, and the vagina tamponed. The after-treatment was exactly like that previously described. The tube was removed on the ninth day, all discharge having ceased. The temperature did not rise above 100°, and reached even that point only on the two days just following the operation. The convalescence of the patient was somewhat retarded by an attack of acute cystitis. The patient returned home September 10th.

She was seen several months afterward, and reported the menstruation to have been perfectly natural. I found the uterus to be likewise normal in size.

CASE 7.—Miss H., consulted me Aug. 6, 1880. She was 33 years old, very anæmic in appearance, and had complained for three years of a gradually increasing menstruation, at this time lasting one week, and amounting to over 30 saturated napkins. There were no clots discharged, nor was there any complaint of dysmenorrhœa. She apparently did not gain during the intermenstrual periods the strength lost by the excessive flow, though she expressed herself as feeling very well, but tired between the menses. She suffered somewhat from leucorrhœa. Bi-manual manipulation revealed an anteverted uterus equal in size to the pregnant uterus at three and a half months. The forward displacement was so great that it was with great difficulty that I could pass the probe, which showed the uterine cavity to be four and a half inches deep. The diagnosis of sub-mucous fibroid was made; and, as I was out of town for the summer, I advised the free use of ergot, beginning a week before the expected flow, and increasing its amount during the menstrual period. I also advised her taking iron between the menses. She entered my private hospital October 5, 1880, for the removal of the tumor. She had gained somewhat in strength during the eight weeks that had passed, the ergot serving partially to control the flow. The cervical canal was dilated with sponge and tupelo-wood tents. Oct. 6th, with the help of my two assistants, she was etherized, the tents removed, and the tumor found growing from the anterior and left lateral walls of the uterus. In size it was equal to a goose egg. Its enucleation was very much hastened by Thomas' scoop; in fact, I had never before been so strongly impressed with its great value in these cases.

The greatest difficulty in the operation was, after enucleation, to remove it through so small a cervix and vagina without tearing them. It was finally accomplished, however, and the drainage tube adjusted as heretofore described. The after-treatment was also like that in the other cases; and all went well until the fourth day, when the temperature began to rise and on the fifth day reached 103.5° . At my afternoon visit the patient was delirious. She had no chills, and there was a constant discharge from the vagina of disintegrating tissue and shreds. Intra-uterine injections had been kept up two and three times daily since the second day; and there was apparently no cause connected with the drainage, for



FIG. 6.

the alarming state of the patient. Yet so sure was I of retention of decomposing fluids in the uterus, that she was put upon the table and Sims' speculum introduced, when it was found that the swelling of the lips of the cervix had quite overlapped the edges of the tube all around; and, although the central opening of the tube was patent into the cavity of the uterus, yet any fluid collecting about the outside of the tube could not be discharged. (Fig. 6.) The two silver sutures which held the tube in place were therefore removed, and the tube taken out. At once there escaped a full half ounce of highly offensive fluid. An ordinary double silver catheter was then adjusted and secured to the external parts. It was through this that the nurse subsequently gave the intra-uterine injections. The patient became restful, the delirium disappeared, and within twelve hours the temperature dropped to 99° . It was, however, necessary to retain the silver catheter for thirteen days, as there was some discharge up to that time. There was no after-interruption to recovery, and the patient was sent home the 29th day of the same month in which she entered the institution. She was examined on Dec. 18, 1880. The uterus was found normal in size, the probe passing $2\frac{5}{8}$ inches. Felt bi-manually, it was also natural in size. The patient reported that since the operation the menstrual flow had continued for three days only, and amounted to but twelve napkins, eight only of these being saturated. She has been heard from several times since and continues to do well.

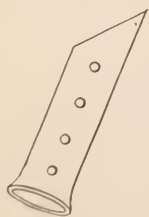


FIG. 7.

In order to avoid in future the difficulty which arose in this case, I then changed the shape of my drainage tube by rounding a flange on its outer end, and later, at the suggestion of Dr. Minot, the sides of the tube were perforated. The instrument as perfected is illustrated by fig. 7.

These cases, excluding the third, (which was introduced simply to show one of the difficulties I have found it necessary to overcome in perfecting the drainage tube) have

several features in common, among which may be mentioned :

All the tumors were myo-fibromata.

All were attached to the uterus over the greater part of one half of their surface.

All were in the body of the uterus, the os internum remaining undilated by any pressure of the tumor upon it.

In all, the prominent symptom was excessive menstruation or hemorrhage.

All the patients were between the ages of 32 and 42. In all, the operation was successfully done without losing more than an ounce of blood ; without incising or tearing the cervix uteri or the vagina ; without inverting any portion of the organ by the continued traction ; without the subsequent establishment of pelvic peritonitis or cellulitis ; and without the loss of life as the result of the operation.

In all, the drainage tube, in its more or less modified form, was used.

The necessity of providing for thorough drainage and intra-uterine injections has been forcibly impressed upon me by the following clinical facts which have come to my observation :

That, in the natural condition of the organ, when there is an obstruction to the canal caused by a flexion, the intensity of the muscular contractions might be able to overcome the bend sufficiently to expel the blood clot, retained menstrual flow, or, at times, excessive uterine secretions ; but when there is a decomposing fluid or pus in its cavity, there seems to be present a state of atony whereby the contractile power of the uterus is lost, causing utter inability on its part to get rid of the purulent material.

That, in a large proportion of the cases of sub-mucous fibroids, the patients are greatly debilitated by prolonged hemorrhages, and wanting in both physical and nervous

tone. There exists a laxity of muscular fibre, partly due to diminished nutrition and partly to a diminished nervous stimulus, the result of which is, both a want of firmness in the structure of the uterus allowing a flexion and obstruction to the canal after the tumor is removed, and also a more ready absorption from its cavity on account of the greater looseness of its tissues.

That, whereas the contractile power of the uterus is undoubtedly greatly increased by the traction and the manipulation necessary in the removal of the tumor, yet after the operation, owing to debility, there is oftentimes present a state of utter inaction on the part of the uterus, rendering us powerless, despite ergot or stimulants, to arouse sufficient contractile power to expel a retained fluid from its cavity.

These cases are very different from those of ordinary labor, where the muscular fibre is developed, and where the nervous stimulus, if wanting, may be supplied by stimulants or electricity.

In the case of an ordinary fibroid, the force to be aroused by the stimulus is largely wanting.

These remarks would likewise apply to the removal of other growths than fibroids from the uterine cavity, as in Case 3, or in the removal of a large mass of cystic degeneration of the chorion, or any growth where there is to be any breaking down or disintegration of tissue causing a subsequent foul discharge.

The time to provide for this drainage is when the operation is done. The canal is then well open and the tube readily adjusted; and the delay required for its introduction does not amount to more than one or two moments. Indeed, there is no good reason why drainage should not be instituted in every instance of the removal of a submucous fibroid wholly within the cavity of the uterus. The

tube does not prevent perfect involution ; for in each of the cases here reported, except Case 2, where death occurred from embolism, the uterus returned perfectly to its normal size, a fact verified both by bi-manual examination and by the passage of the uterine probe. Nor does its presence induce subsequent hemorrhages, there being no such history in any case where it was used. Let the surgeon who prefers to wait for symptoms of septicæmia before he provides suitable drainage or means of irrigation but once be forced to contend with the difficulties that are almost sure to arise—difficulties in the adjustment of the tube on account of the weakness of the patient, her sensitiveness to the touch, or her highly nervous condition ; difficulties caused by constant blocking up with shreds of tissue on account of the small-sized canula which, at that late hour, he is obliged to use ; or, if not actual difficulties, many slight vexations—I say, let him have but one such case to treat, and I am sure he will agree with me in the care of all subsequent ones.

My ideas regarding the removal of sub-mucous fibroids wholly within the cavity of the uterus, have undergone the most decided change in the last six years, in that then my advice was either to dilate repeatedly with tents, or at once incise the cervix and the capsule and give ergot, delaying all attempts at removal until the tumor presented at the os externum. In this treatment, I should have followed the teachings of Sims or Emmet ; but I should also, I am sure, have subjected the patient, in thus doing, to months, perhaps years, of unnecessary suffering and possible hemorrhage ; and also to needless dangers from the repeated use of tents, or from cutting the cervix or the tumor itself. Either of these processes I look upon as more serious than the actual removal of the tumor, the dilatation of the cervix preparatory to enucleation always being, to my mind, the most dangerous part of the whole work.

Now, in any case of suspected fibroid within the uterine cavity, I should advise immediate dilatation. This should be done in the inter-menstrual period, and as thoroughly as possible, using first a sponge tent, which seems best to soften the tissues; and substituting for it, after 8, 10, or 12 hours, as many laminaria or tupelo-wood tents as can be crowded through the os internum. The last-named tents do not expose the uterus to the prolonged septic influence that the sponge is apt to, if used throughout the whole process of dilatation. In as many more hours I should etherize the patient and explore the cavity with the forefinger. If the presence of a tumor be thus verified, remove it at once, even though it be small and project but slightly into the cavity, as in Case 4; for the danger of inflammatory action is greater in leaving it after what you have already done, than in pushing on and taking away the cause of the difficulty. If dilatation by the tents sufficient to allow the passage of the forefinger be not secured, I believe there is less risk in rapid dilatation with the hard-rubber plugs referred to, than in delay and the subsequent repetition of tents.

The practitioner is very apt to compare the process of dilating the non-pregnant uterus to that where pregnancy exists; a process with which he is generally more familiar, but which is totally different from it. In the one case he has to deal with a rigidity and resistance, particularly if the patient has never borne children, requiring a tremendous force to soften and overcome, and this force to be continued often for a long time. In the other he has the uterus softened by the influence of pregnancy, whose tissues yield readily to the application of a comparatively mild force for a short time.

Many authors advise the *écraseur* for this operation; but in my experience it has proved a cumbersome, annoying,

and worthless instrument, and I have twice been called to remove the remnant of a fibroid which the previous surgeon, using this instrument, had left behind. There is also greater danger, than with scissors, of cutting off a portion of a partially inverted uterus. As to the danger of hemorrhage and the advantages of the *écraseur* in controlling it, I do not believe, if Emmet's method of traction be practised, that such danger exists. I certainly have never seen any trouble from hemorrhage, either in my own practice or that of others, where this method could be used and the growth cut away with the scissors.

I would by no means have it understood that I advise drainage in every case of the removal of a fibroid. It must of course be limited to intra-uterine tumors, and is obviously non-essential in some of these; as, for instance, a fibroid polypus whose attachment to the uterus, even though it be to some portion of the interior of the body, is so small that the resulting denuded surface will be insufficient to give rise to any serious complications. Nor is it generally necessary when the fibroid has fully dilated the os internum, as the contraction of the cervix is not usually established until most of the disintegration has taken place. In the exceptional instance in the case of Dr. Sims, however, cited at the beginning of this paper, where the amount of tissue or shreds necessarily left is considerable, the practice is to provide drainage.

And I would, in conclusion, again strongly urge the use of the drainage tube after the removal of sub-mucous fibroids wholly within the uterine cavity, to be adjusted at the time of the operation, sewed to the cervix, and left in place till all discharge from the uterus has ceased.

NEUROSES OF SENSATION OF THE PHARYNX AND LARYNX, OR SENSORY NEUROSES OF THE THROAT.*

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SECTION I.—DEFINITION.

NEUROSIS of sensation of the throat¹ is defined to be functional—and not merely subordinately symptomatic—disordered sensibility of the throat. (I mean to use the word *functional* neither “unphysiologically” nor “irrationally”: it does not imply with me that there is no physical basis for the particular derangement of function, but that the structural change is either remote from the organ the function of which is deranged, or that it is molecular and not discernible with the means of observation at our command.) The definition is meant to exclude, on the one hand, cases of neuritis and observed alteration in nerve tissue of the throat; and, on the other, throat neuroses which have no other than a symptomatic significance, as the increased sensibility and painfulness in inflammatory

* By special appointment prepared for, and partially read before, the International Congress in London.

¹The term “throat” is here used technically, to denote all the parts which at the present day the throat-specialist, or laryngologist, is called upon to treat: *par excellence*, therefore, the larynx and pharynx.

affections, the diminished or abolished sensibility during narcosis and hypnotism, in anæsthetic lepra, and in connection with the failure of all the powers, such as during an epileptic fit, in cholera, and in approaching death from any cause, and the perverted sensibility occasioned by the actual presence of foreign bodies, tumors, etc., as well as the misinterpretations of sensibility which result from delusive conceptions in primary insanity.

SECTION II.—ANATOMICO-PHYSIOLOGICAL BASIS.

Many questions relating to the normal and pathological sensibility of the larynx, pharynx, and the other constituents of the throat are not yet definitely settled.

It is well known that normal sensation involves the integrity of three factors, viz., of the nerve ends which receive an impression, of the conducting substance which conveys the impression, and of the central organ which receives the impression afterward transformed into conscious sensation. My definition of sensory neuroses of the throat excludes the consideration of structural throat lesions, involving the peripheral sensitive nerves, and of the brain lesions of insanity. In all other cases, there must be affected either the extra-axial or intra-axial nerve tracts or the nuclei immediately connected with them. Investigators agree that the nerves with which we are concerned are mainly those that go to make up the pneumogastric, and, as to the fauces and pharynx, to a large extent, also, the glosso-pharyngeus. As is well known the larynx obtains its nerves of sensation almost exclusively from the superior laryngeal nerve which emanates from the plexus gangliformis of the pneumogastric,—a plexus in which enter, besides the vagus, fibres from the accessory and hypoglossal nerves, and which receives anastomotic branches from the glosso-pharyngeus, the first cervical ganglion of the sym-

pathetic, and the cervical plexus. There are a few sensory filaments contained in the inferior laryngeal nerve, which are given off with the nervus arytænoideus,—the terminal of one, namely, the external, of two branches (sometimes sets or bundles) into which the inferior laryngeal nerve divides. The immediate source of the sensory nerve supply of the pharynx is the pharyngeal plexus which is composed of the pharyngeal branches of the pneumogastric with which are mixed the pharyngeal branch of the glosso-pharyngeus and the sympathetic. The pneumogastric in connection with the glosso-pharyngeus supplies the inner sides of the palatine folds, the fauces and tonsils as well as the posterior side of the velum palati. The second branch of the fifth supplies the anterior face of the velum and the region of the pharyngeal orifice of the Eustachian tube and vault of the pharynx.

While, thus, the derivation of the sensitive nerves of the throat from these larger nerves is clear, of their connection through the nerve roots with their immediate centres, nothing positive is known. It has, however, been suggested to me by Dr. E. C. Spitzka, of New York, in a personal communication, that by a process of anatomical exclusion we may infer where their nuclear centre probably is. His reasoning is as follows: Taking the ninth, tenth, and eleventh pairs of cranial nerves, *i. e.*, the glosso-pharyngeal, pneumogastric, and spinal accessory, in the aggregate, it is found that they have in common three nuclei. These are, 1. a *sub-ependymal* nucleus, which experiment as well as several anatomical facts justify us in regarding as a nucleus for visceral innervation. 2. A large multipolar-celled nucleus, devoted presumably to the innervation of the laryngeal muscles. (Certain authors incline to the belief that this is really an origin of the hypoglossal nerve¹; and

¹ Laura, Mem. della reale academie delle scienze di Torino, serie II, j. 31 and 32.

Krause¹ considers its relations so much in doubt that he terms it *nucleus ambiguus*, but there are no good reasons for refusing assent to the proposition of Meynert² that it contributes its efferent fibres to the oblongata-portion of the eleventh pair, in other words, to that part of these nerves which through its subsequent fusion with the tenth pair innervates the muscles of phonation.) 3. The *nucleus pharyngeus*,³ a mass of cells situated near the lateral field of the oblongata, which from its multipolar cells and the fact that it is found in best development in the levels of the ninth and tenth pairs is supposed to be devoted to the innervation of the pharyngeal muscles.

There is a fourth nucleus, viz., the gelatinous substance with small nerve cells scattered around the ascending root of the fifth pair. Now, as the sub-ependymal nucleus is probably visceral and the deep nuclei in all likelihood motor, the sensory innervations naturally fall to this nucleus, which is documented as a tactile and trophic centre by its relations to the great cranial tactile nerve, the fifth.

A fact which certainly strongly points to this conclusion is that while the spinal accessory nerve roots never pass through or receive accession from this fourth nucleus, the ninth and ten pairs do pass through and do receive such accession.⁴ Here then we may look for the tactile innervations of the larynx and pharynx, and the field from which certain reflexes are mediated to the motor nuclei. The analogy with the spinal reflex structure would thus be complete; for the gelatinous head of the posterior horn of the cord being homologous with the gelatinous nerve nucleus

¹ Krause, Allgem. und mikroskop. Anatomie. Hanover, 1876.

² Meynert, Vom Gehirn der Säugethiere. Stricker's Handbuch.

³ Spitzka, "Architecture of Brain," *Journal of Nervous and Mental Diseases*, 1880.

⁴ Meynert, *op. cit.*; Stilling, "Ueber die Textur der Medulla oblongata," Erlangen, 1842.; Spitzka, *loc. cit.*

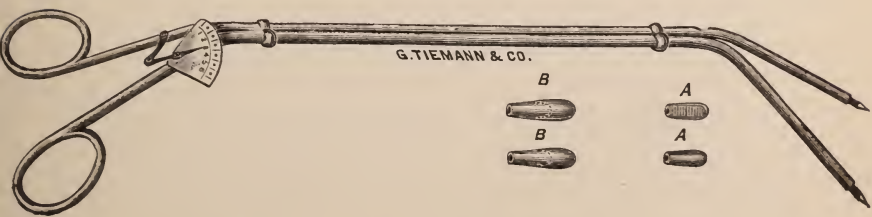
of the trigeminal root, the nuclei of the pharyngeal and laryngeal muscle-nerves being analogous to the anterior cornu and their efferent fibres analogous to the anterior root, the afferent fibres of the ninth and tenth pairs represent so many posterior root-fibres and complete the reflex arch. No opportunity for an autopsy where this nucleus might have been affected has occurred to me since Dr. Spitzka's suggestion; but in a case, which has turned out to be progressive paralytic dementia, in which for some time disease seemed to be confined mainly to this domain, we may possibly in the future have light thrown on this point by *post-mortem* examination.

The normal sensibility of the throat varies in different portions and in different persons, even at different times in the same person; nevertheless, there is some agreement within recognizable limits. The exceptional cases of hypersensitiveness and of want of sensitiveness met with, within the latitude of health, in performing laryngoscopy and various surgical operations, are not sufficient to disturb the rule. Excessive variations must be regarded as being abnormal.

The only published investigations as to the sensitiveness of the laryngeal mucous membrane are those of Pienaczek.¹ He found that temperature was distinctly appreciated; that while tactile perceptions were usually covered by the irritation which the touch of bodies caused, yet when habit had blunted the irritability and the reflexes were restrained by the parts having become accustomed to the touch, differential tactile appreciation seemed to take place; and that, the perception of pain was much less in the larynx than in the skin. After repeated similar examinations by means of various probes, blunt-pointed and sharp, and all sorts of throat instruments, metallic and of different materials, I

¹ Über die Empfindlichkeit der Larynxschleimhaut." *Medizinische Jahrbücher*, Vienna, 1878 p. 481.

can express myself in entire accord with Pienaczek, as far as he goes. I found the appreciation of temperature acute in every portion of the throat; next in distinctness of sensation were differences in pressure, and thereby, to a slight extent, consistence and form. Without going deeply into physiological disquisition, I must recall the interesting observations on acuteness of sensibility of different portions of the body made nearly half a century ago by E. H. Weber.¹ He measured the degree of sensibility by the distance between the two legs of a pair of compasses which different parts require in order that the two points make distinct impressions. Aside from the skin he examined the mouth, and found that at the tip of the tongue the two points were separately felt when only half a line apart, while on the dorsum and on the edge of the tongue (one inch from the tip) they had to be four lines apart, and that the mucous membrane of the hard palate required the distance to be six lines, and over the gums nine lines, before the sensation of two points and not of one was produced. Weber assumed that the difference of sensibility depends upon the number, course, and termination of the nerve filaments in the different portions. On account of the irritation, which touching the throat with any instrument usually produces, it is very difficult to arrive here at any accurate æsthesiometrical results. Somewhat after the model of Ziemssen's double laryngeal electrode, I have devised a con-



¹ *Annotat. Anat. et Physiol.*, pp. 44-81. Quoted by Dr. Johannes Müller, "Handbuch der Physiologie des Menschen." Coblenz, 1837, vol. i, p. 711.

venient throat æsthesiometer, which, as shown in the accompanying wood-cut, essentially consists of two rods properly bent, so connected together that their points can be easily approximated and separated, the distance being measured on a scale at the handle. A little way from the extremities, screw-threads commence for the purpose of carrying tips, some of which are metallic, marked *A* in the figure, while others are made of very soft rubber, marked *B*.

From investigations in a number of healthy persons I can state that the normal distances between two uncovered points of the æsthesiometer necessary for their distinct appreciation vary from $1\frac{1}{2}$ or 2 to $2\frac{1}{2}$ or 4 centimeters. This, of course, precludes the possibility of measuring by such distances the sensibility of very circumscribed spots. Besides, practice changes the distance, and the examination is by no means easy. More important than the examination of the distances is that of temperature and pressure. Normally very slight differences of temperature are appreciated. I examine these with my æsthesiometer, the two points mounted with metallic tips, each dipped into water of a different temperature (or otherwise heated and cooled), thus constituting a differential thermæsthesiometer. To measure appreciation of consistence, etc., *i. e.*, pressure, I use the soft-rubber tips. The application of electricity furnishes the best means of measuring the sensitiveness to pain. I discriminate between three different kinds of normal as well as abnormal sensibility of the throat, viz.: 1. *tactile*, by which temperature and pressure are appreciated; 2. *dolorous*, by which pain is appreciated; and, 3. *reflex*, from which result muscular contractions, such as cough, spasm, gagging, choking, etc., as well as intravascular and secretory phenomena. Each of these kinds of sensibility must be examined æsthesiometrically; the first, as I have above described; the second, by an elec-

trode, connected first with an induction machine and then with a constant battery, and also by a sharp-pointed probe, or my æsthesiometer uncovered; and the third by a blunt-pointed probe, or my æsthesiometer covered. Under the head of diagnosis I shall speak again of this examination, and especially of the examination of dolorous and reflex sensibility.

As to what I call dolorous sensibility, there has been some discussion whether or not the sensation of pain ought to be considered as a function of sensitive nerves. Anstie¹ insists that it ought not; but in spite of his verbal distinction that "it is not the *function* of sensitive nerves to convey the sensation of pain but only their *action* under the presence of extraordinary influences," I think it may be stated without fear of serious contradiction, that to mediate the appreciation of a certain amount of pain in response to appropriate impressions is their normal function, and that any alteration of this mediation, whether a diminution or an increase, beyond the limits of individual healthy variation, as well as a spontaneous sense of pain, constitutes a disorder of sensibility, *i. e.*, a dysæsthesia.

Krishaber² was the first who drew attention to the fact that the sensibility of the larynx is of two kinds, viz.: a common or general sensibility (which includes the two kinds I call tactile and dolorous) and a reflex or special sensibility, and that a sensory disorder may affect one or the other of these, or both; but I do not agree with him as to his definition of laryngeal reflex sensibility. He pointed out that if we cauterize the mucous membrane of the larynx, we provoke a number of noisy and painful inspiratory movements, convulsively closing the larynx, while expiration remains calm and deep. There is pain for some hours or minutes

¹ "Pharyngeal and Laryngeal Neuralgia." *Neuralgia and the Diseases that resemble it.* New York, ed. 1872, p. 113.

² *Dictionnaire Encyclop. des Sciences Méd.*, Dechambre. Paris, 1872, 2me serie, vol. i, p. 677.

according to the strength of the caustic, but *no cough*. If, instead of cauterizing, we carry a simple drop of water to the part, thus imitating what frequently happens in so-called "wrong swallowing"—swallowing the wrong way,—we also provoke convulsive movements of the laryngeal muscles, but the phenomena are entirely different from those in the former case. The most prominent phenomenon now is a violent cough, a cough hard and loud, for the production of which all the expiratory muscles are called into action; and while inspiration, though in the beginning of the paroxysm perhaps much embarrassed, soon becomes calm and normal, the cough continues as long as there is the least disagreeable sensation in the larynx. Only to the sensibility excited in the latter case Krishaber accorded the name reflex sensibility; that in the case of cauterization he regarded as the general or common sensibility; the first being followed, as he said, by tickling and cough, the latter by pain and local spasm; the first a special sensibility relating to an occasional function, the expulsion of matters from the air passages, being connected with expiration; the latter a general sensibility relating to a permanent function, the permeability of the *rima glottidis* (the perturbation of which can give rise to a formidable accident, closure of the air passages by spasm), being connected with inspiration. Now, although the discrimination which Krishaber made between expiratory and inspiratory convulsive movements is an important one, both phenomena are reflex. Not only both cough and spasm, but, as I have already stated, other muscular contractions and circulatory and secretory actions may result from excitation of the reflex sensibility of the throat.

Taking the three kinds of sensibility together, I have found in two persons out of three—and at my request Dr. Ephraim Cutter examined his own very tolerant throat and arrived at substantially similar results—that the angles of

the posterior wall of the pharynx just behind the posterior palatine folds, and the posterior wall of the larynx, are the most sensitive portions of the throat. Next comes the laryngeal face of the epiglottis, while the edge of the velum is the least sensitive. The different portions vary in the following order, viz.: 1. Angles of pharynx above described. 2. Posterior laryngeal wall and inter-arytenoid fold. 3. Laryngeal face of epiglottis. 4. Valleculæ and root of tongue. 5. Lower laryngeal cavity (when reachable). 6. Trachea (when reachable). 7. Lateral laryngeal walls. 8. Ventricle of Morgagni. 9. Arytenoid cartilages. 10. Pyriform sinuses. 11. Palatine folds. 12. Tonsil. 13. Glosal face of epiglottis. 14. Free edge of epiglottis. 15. Ary-epiglottic fold. 16. Fauces. 17 Lateral walls, and low down on the posterior wall, of pharynx. 18. Vocal bands. 19. Uvula. 20. Velum, on the sides. 21. Velum, in the centre and on the edges.

(To be continued.)

TRANSIENT ALBUMINURIA, AS IT OCCURS, PARTICULARLY IN CHILDREN AND ADO- LESCENTS, IN APPARENT HEALTH.*

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THE occurrence of a transient albuminuria in persons in apparent health, and particularly in children and adolescents, is a subject which has attracted the attention of numerous clinical observers and pathologists during the past few years, and is certainly one of much practical importance. Cases have been reported by the English physicians Sir William Gull, Drs. George Johnson, Moxon, Saundby, Dukes, and T. Morley Rooke, and by Leube, Fuerbringer, Edlefsen, and Ultzmann among other German observers.

Before further reference to and analysis of their observations and a number of my own, I shall briefly review the various theories which have been advanced in explanation of the conditions which govern this form of albuminuria. Accepting Nussbaum's¹ conclusions, based upon experimental research, that the site of transudation of albumen in the

* Read before the New York Academy of Medicine, Dec. 15, 1881.

¹ Nussbaum, M.: Fortgesetzte Untersuchungen über die Secretion der Niere; *Pflüger's Arch. f. d. gesammte Physiologie der Menschen und der Thiere*, 1878, xvii, 581. Ueber die Entstehung der Albuminurie; *Deutsche Arch. f. Klin. Med.*, 1879, xxiv, 248.

kidney is in the glomerular vessels, Runeberg¹ maintains, as the result of numerous experiments by himself, that the filtration of albumen is dependent upon an increased permeability of the walls of these vessels, due to a condition of diminished or low blood pressure. The filtration membranes used by Runeberg in his experiments were dogs' sheeps' and rabbits' intestines, suitably prepared. Heidenhain asserts that Runeberg's conclusions are untenable in view of the demonstrable faultiness of his experiments. He shows by an analysis of his results, "that while the percentage of albumen in the filtrate diminishes by increased pressure, the absolute quantity of albumen is actually increased. With increased pressure both more albumen and more water are filtered, but the albumen stream increases more slowly than the water stream, so that the percentage of albumen in the filtrate relatively decreases with heightened pressure."²

He also shows that the membranes used by Runeberg differ in their filtration properties from the glomerular membranes, and that in other respects there is an essential difference between "the physical experiment and the physiological process."

Wittich has advanced the theory that the urine which passes through the glomerular capillaries constantly contains albumen, but that this albumen serves the purpose of nourishing the epithelium of the urinary tubules, and that the residue passes back into the circulation. In a diseased condition of the epithelium, or in its removal from any cause, albuminuria would occur. An objection which has been urged against this hypothesis, is that it assumes a different mode of nourishment of the renal epithelium from

¹ Runeberg, J. W.: Die Filtration von Eiweisslösungen durch thierische Membranen; *Arch. f. Heilk.*, 1877, xviii, 1. Albuminurie bei gesunden Nieren; *Arch. f. Path. Anat.*, 1880, lxxx, 175.

² Heidenhain, R.: Hermann's "Handbuch der Physiologie," 1880, Bd. v, 367.

all other epithelium, while it fails to satisfactorily explain the transient albuminuria following an epileptic attack, occasionally cold bathing, and occurring in the cases recorded in the present paper. Rosner, moreover, has adduced an experimental objection to this theory. By placing fresh bits of kidney in boiling water, he was able to fix the albumen in the situation in which it was produced. In diseased kidneys coagulated albumen was found in the urinary tubules, but never in healthy organs. Another explanation, which has been suggested, is that the epithelial covering of the smallest capillaries prevents the transudation of albumen; denuded of their epithelium, albumen escapes.

The experiments of Stokvis and others would seem¹ to demonstrate the incorrectness of the theory that the filtration of albumen is dependent solely upon a condition of increased or high pressure in the glomerular vessels. Stokvis has shown that when the pressure is increased in the renal arteries either by ligating the aorta below their origin, or through the extirpation of one kidney, albuminuria does not occur until the degree of pressure is such as to rupture the vessels and permit of the admixture of blood with the urine. The clinical observations that in the case of the small kidney, which is so constantly associated with high arterial tension, albuminuria as a rule occurs only occasionally and to a limited degree²; and, again, that in those forms of heart disease attended with obstructed venous circulation, albumen does not appear in the urine until the general arterial tension, and, consequently, that in the glomerular vessels, is lowered, through failure in the heart's action, while remedies which directly

¹ "Cyclopædia of the Practice of Medicine," Ziemssen, Supplement, 1881, p. 661.

² Mahomed's explanation of the diminished filtration of albumen in this condition, on the ground of a maintenance of balance between thickness of vessel walls and arterial pressure (*Lancet*, vol. i, p. 76, 1879), I cannot accept without reservation, in view of some personal observations, which I hope, at another time, to report.

tend to increase its force diminish or arrest the albuminuria, are in accord with the physiological experiment.

Other theories to which the transudation of albumen have been referred are a retardation of the blood current in the renal vessels¹ (Heidenhain and others), and Leube's hypothesis² of an individual permeability of membrane. It was shown in the experiments of Heidenhain and Runeberg (*l. c.*) that the filtration of albumen through animal membranes goes on very slowly; in other words, that it is an essential condition of such filtration, that the albumen should remain a comparatively long time in contact with the vessel wall. A vaso-motor disturbance within the kidney (either a paralysis of the vaso-constrictors or an irritation of the vaso-dilators), by producing a dilatation of the vessels, would fulfil this condition, viz., a retardation of the blood current. That this condition should alter the nutrition of the glomerular epithelium, and, when temporary, should produce a transient disturbance of the functions of the latter, is readily conceivable. It has seemed to me that the above theory, viz., a slowing of the blood current in the glomerular vessels, dependent upon a temporary vaso-motor disturbance, with a resulting alteration (also temporary) in the functions of the glomerular epithelium, may be regarded as the most probable explanation of the mode of production of transient albuminuria in persons presumably healthy. It remains for us to seek for the cause of such a vaso-motor disturbance. Inasmuch as my observations have been confined chiefly to children and adolescents, in whom, from the time of life, apparent health, and disappearance of the albuminuria (in the cases recorded), structural disease of the kidney may reasonably be excluded, I shall not venture to extend my conclusions to a later period in life.

¹ *Loc. cit.*

² Leube, W.: Ueber die Ausscheidung von Eiweiss im Harn des gesunden Menschen; *Virchow's Archiv*, 1878, lxxii, 145.

From a careful analysis of cases recorded by other observers and a study of my own, I have been led to believe that the source of irritation, in a large number of instances, is to be found in the temporary presence of imperfectly oxidized nitrogenous matters in the renal circulation; in other words, to a transient oxaluria or lithuria, which, clinically, may be regarded as identical. The marked derangements of the nervous system in more or less chronic lithæmia in adults are well recognized; many of the symptoms which Dr. Da Costa¹ has recently made the subject of a very interesting monograph indicate the degree to which the vaso-motor system is affected. In children, the general nervous symptoms attending the lithæmic state are equally pronounced, though hitherto not as well recognized. To this fact my attention was first drawn by Dr. Wm. H. Draper, and my experience since has fully corroborated it. That the nervous symptoms are directly dependent upon the presence in the blood of the irritating products of imperfect oxidation of nitrogenous matter can hardly be doubted, although different views may be held as to the system which is primarily concerned in the production of this state.

With these facts before us we may reasonably believe that a more or less transient oxaluria or lithuria may be capable of producing a correspondingly temporary irritation of the nervous system. A transient general nervous disturbance, in which the vaso-motor system would necessarily share, may be supposed, or one confined to the vaso-motor system of the kidney in its excretion of a large amount of irritating matter, and resulting in a temporary albuminuria.

A similar albuminuria which follows an epileptic attack, may presumably be referred to the disturbance which affects the general nervous system, and consequently, the vaso-motor system of the kidney.

¹ Da Costa, *American Journal of the Med. Sciences*, Oct., 1881.

Again, in the temporary albuminuria which has been observed in cases of exophthalmic goitre,¹ we have a probable instance of retarded blood current in the renal vessels, the result of dilatation dependent upon the affection of the general vaso-motor system.

The apparently more frequent occurrence of temporary albuminuria, without structural change in the kidney, in children and adolescents, than at a later period, I would explain on the ground of the greater mobility of the nervous system which obtains at these periods of life.

Its occurrence in only a comparatively small proportion of cases of lithæmia at these ages may be referred to an *individual* mobility of the nervous system in such cases. (In another group, the skin or the mucous membranes may be the most vulnerable portions of the organism.) In my own experience, a transient albuminuria in connection with the lithæmic state has been observed, as a rule, in children and youths of nervous temperament; in the latter instance, in active brain-workers, frequently during periods of unusual mental worry or strain; under conditions where the general nervous tone was below par. (In this connection a simple reference to the clinical fact of the influence of profound mental emotion in temporarily increasing the albuminuria in chronic Bright's disease is suggestive.) Leube's hypothesis of an individual permeability of membrane (*l. c.*) may also be considered as a possible etiological factor in these cases.

In illustration of the subject which we have been considering, I shall now briefly review the cases of transient albuminuria which have been recorded by various observers.

In a discussion before the Royal Medical and Chirurgical Society of London in 1873, on albuminuria,² Sir William Gull mentioned that in his experience "it occurred in

¹ Dr. Begbie, *Edinburgh Med. Journal*, April, 1874. ² *Lancet*, 1873, i, 808.

young and growing men and boys, almost as frequently as spermatorrhœa," and that it might presumably be referred to an atony of vessels and nerves. In a paper by Dr. Moxon ("Guy's Hospital Reports," 1878) 19 cases of intermittent albuminuria occurring in adolescents in apparent health are reported. The symptoms presented by the patients were of an indefinite character, consisting in listlessness, languor, occasional headache, unrefreshing sleep, with little disposition for cheerful company. In the majority of cases no complaint was made by the patients, but advice was sought through the anxiety of friends. Dr. Moxon says: "I have not met with this set of conditions at other periods of life, and I have so frequently met with them in adolescents, that I cannot but believe that it is a disordered state proper to that term of life, and that it is deserving of recognition and receiving a name." He further mentions that careful examination, in his cases, of the various organs of the body gave negative results, and that, moreover, he was able to exclude scarlatina and diphtheria, which might leave a sequel of albuminuria and lead to fallacy. Many of the cases were kept under observation for several years, the urine being repeatedly examined, with the result of finding that the albuminuria had wholly disappeared, and that excellent health was maintained.

In the history of the various cases, whenever the result of the examination of the urine, aside from the existence of albumen, is recorded, the presence of a large amount of oxalate of lime, or a combination of uric acid and oxalate of lime, is mentioned. Their presence, and in large quantity, was so commonly observed as to attract Dr. Moxon's attention. He does not, however, express any positive opinion as to their causal relation to the albuminuria. It may be mentioned that small oxalate of lime calculi were subsequently passed by one of his patients.

In the *British Medical Journal* of Nov. 10, 1878, ten cases of intermittent albuminuria in adolescents from 13 to 17 years of age are reported by Dr. Clement Dukes. In three of the cases there is presumptive evidence of the existence of structural change in the kidney. In Case 1 the albuminuria followed an acute attack of nephritis with bloody urine, and occurred at intervals for the subsequent eight months, when the record ceases.

In Case 2 albumen was detected in the urine immediately subsequent to an attack of scarlatina, and it continued to appear from time to time up to the date of publication of Dr. Dukes' article.

Finally, in Case 10 it is stated that with pallor and a feeling of faintness on severe exertion, high arterial tension existed, and an examination of the heart revealed a heaving, hard impulse—suspicious symptoms certainly in connection with albuminuria. Inasmuch as in only three of the remaining cases had the albuminuria disappeared at the date of record, and in these for a comparatively short time, we are hardly warranted in positively assigning them to the group which we have under consideration. They certainly differ in many respects from the cases described by Dr. Moxon, and from my own. The symptoms consisted of furred tongue, headache, disinclination for work or play, irregular or constipated bowels; often there was anæmia, and occasionally a slight cold, facial dropsy, and slight synopal attacks. The microscopic examination of the urine is not given. In Dr. Dukes' opinion the albuminuria is the result of a temporary hyperæmia of the kidneys, super-added to an habitual increased arterial tension, which he claims obtains at puberty, basing this opinion upon Beneke's researches. Mahomed, on the contrary, asserts that the result of his personal experience is to the effect that a condition of low tension exists at this period of life.¹

¹ *Lancet*, 1879, vol. i, p. 76.

In an article in the *British Medical Journal* of Nov. 12, 1881, Dr. Dukes reverses his former opinion, stating that as the result of further observation he is forced to regard the albuminuria of adolescents not only as pathological, but as representing the beginning of true Bright's disease. He adds, however, that he has been unable to observe his cases in its further development.

In a single case of transient albuminuria recorded by Dr. Yeo (*British Med. Journal*, Oct. 26, 1878), the presence of albumen in the urine alone being mentioned, an explanation of the symptom is sought in the hypothesis of a temporary vascular asthenia, dependent upon a possible deficiency or disturbance of nerve force of transient duration.

In an intermittent albuminuria in a young girl, disappearing during rest in bed,¹ Dr. T. Morley Rooke seeks to explain the condition on mechanical grounds. He says that "when the body is in the upright position the weight of the column of blood is too great for the weakened vessels,"—a theory which requires further demonstration.

In a very interesting paper in the *British Med. Journal* of Dec. 13, 1879, on "Latent Albuminuria, its Etiology and Pathology," Dr. George Johnson mentions the frequency with which albumen is found in the urine of persons in apparent health, and terms it "latent albuminuria." He maintains that although unassociated with any evidence of functional or structural disease, it may, by careful inquiry, be traced back, in a very large proportion of cases, to some possible exciting cause, *i. e.*, to an attack of scarlet fever or diphtheria, or a cold, etc.; moreover, "that even the smallest trace of albumen in the urine is always pathological, never physiological; and that the neglect of a pathological condition and tendency, especially such negligence

¹ *British Med. Journal*, Oct. 19, 1878.

as involves repeated exposure to the exciting cause, may convert a temporary and occasional into a persistent albuminuria, which sooner or later, though it may be after many years, will result in a fatal disintegration of the kidneys." Admitting the frequent dependence of albuminuria upon dyspepsia, he believes that the chain of events is represented by an irritation of the gland cells of the kidney in the process of excretion of ill-digested matter, through which they later undergo structural changes, while at the same time the imperfectly assimilated albuminous materials pass more readily by exosmosis through the Malpighian bodies. An additional factor is found in the general nervous exhaustion, with loss of vaso-motor force, which accompanies chronic dyspepsia, leading to a diminution of tone and contractile power in the muscular walls of the arterioles generally, including those of the kidney. Admitting, therefore, the irritating effect of the products of mal-assimilation upon the kidney, as well as a vaso-motor disturbance accompanying their presence in the blood, he believes apparently that the gland cells are primarily affected, undergoing structural change with a continuance of the irritation, rather than that such irritation may be confined to the vaso-motor system within the kidney, with resulting temporary disturbance in the nutrition and functions of the glomerular epithelium, and consequent transient albuminuria.

In an examination of the urine of 145 male patients by Dr. Saundby, taken *seriatim* as they presented themselves at the General Hospital in Birmingham, England, albumen was discovered in no less than 105¹; 67 of these cases are tabulated by Dr. Saundby under the head of chronic Bright's disease; five cases, occurring in patients between

¹ Saundby, R. : The Diagnostic Value of Albuminuria ; *Brit. Med. Journ.*, 1879, i, 699.

ten and twenty years of age, are attributed to simple debility; two, at a similar period of life, to dyspepsia.¹ Of these a portion, he says, resembled Dr. Dukes' cases, a portion Dr. Moxon's.

No evidence of organic disease could be detected. The examination of the urine is not given in full, but it is stated that in a few instances oxalate of lime was present. In Dr. Saundby's opinion a temporary hyperæmia may be regarded as the source of this form of albuminuria, and he believes that it is in no respect inconsistent with an integrity of renal structure.

In a case of intermittent albuminuria, in a physician, 29 years of age, in good health, which finally disappeared after an interval of eight months, reported by Fuerbringer,² there are several points of interest. The albumen was first accidentally discovered in a morning specimen of urine, passed after an hour of great mental anxiety. The noon urine of the same day contained less, and the evening water, after the mental depression had passed away, was entirely free from albumen. During the following week occasional traces of albumen were detected. After a second period of great anxiety and depression a small quantity of dark, clear, highly acid urine was passed, of a sp. gr. 1.030, containing 31 per cent. of albumen and a large amount of uric acid. Under similar circumstances, on two subsequent occasions the same symptoms occurred. At other times, when the amount of albumen was less, uric acid, or oxalate of lime, or both, with amorphous urates, were commonly present. No effect upon the albuminuria was produced by diet. The ingestion of eggs in large number, strong spices and liquors, were followed by negative results. Severe physical exercise produced a temporary in-

¹ In one of the cases classed under the head of debility, the albuminuria followed typhoid fever.

² *Zeitschrift für Klinische Medicin*, 1880, i, p, 340.

crease in its amount. A very careful examination of the patient failed to reveal any organic disease. Fuerbringer would explain the phenomena on Runeberg's theory of diminished pressure in the glomerular vessels, produced by general arterial ischæmia, the result of profound emotional disturbance of a depressing character. The influence, however, of similar causes in exciting a temporary lithæmia is well recognized, and I would therefore suggest that profound mental depression, lithæmia, albuminuria constituted the probable sequence of events in the above case.

The following cases have come under my personal notice. Within the past few years the number of such observations has been comparatively large, but as they have all been made in private practice, the opportunities for carefully tracing the greater number of cases have been wanting. I shall, therefore, with a single exception, confine my remarks to such as I have been permitted to keep under constant observation, and where the albuminuria has now been absent for many months. In all of them careful inquiries were made in regard to previous illnesses, which might have a bearing upon the subject under consideration, with results which will be stated in the individual cases. The possible existence of affections of the bladder and urethra were carefully investigated, but they were invariably found to be absent. In this connection Simon's researches, to the effect that neither the prostatic nor seminal fluids contain any bodies which are coagulable by heat, may be mentioned. "Casts" were never found in the urine, although careful search was made for them. In none of the cases was the amount of urinary water apparently decreased, although the quantity passed in the 24 hours was not measured.

Sphygmographic tracings, which were taken whenever possible, showed either normal or slightly lowered arterial tension. In all the cases the amount of albumen in the

urine was very considerable. In all, the microscopic examination was made within *twelve* hours; in many of the cases, as soon as *two* hours after the passage of the urine.

At the period when the following case was observed my attention had not been directed either to the occurrence of transient albuminuria in health or to its association with a temporary oxaluria or lithuria. I believed it, at the time, to be one of latent Bright's disease.

CASE 1.—Mr. A., aged 23. The patient is of nervous temperament. While in New York on a visit in the winter of 1874, in apparent health, his urine was examined by me in a non-professional capacity. To my surprise and concern it was found to contain a large amount of albumen, together with much uric acid and oxalate of lime. Without mentioning the matter to Mr. A., I immediately wrote to his family physician in regard to the result of my examination. I received in reply a communication to the effect that the urine had been examined by him on Mr. A's. return home, two days later, and was found to be wholly free from albumen. His water has been very frequently examined since with a similar result, and his health has continued to be excellent.

CASE 2.—Mr. B., medical student, aged 21. Is of nervous temperament. Had scarlet fever without albuminuria during childhood. Has never had diphtheria. Has been under my observation for the past five years, during which period he has enjoyed excellent health. In February, 1881, while engaged in active brain work, taking little exercise or recreation, but eating largely, he began to complain of lassitude, weariness after exertion, either mental or physical, and slight morning headache. An examination failed to reveal any evidence of organic trouble. Examination of the urine gave the following results: React. highly acid; sp. gr. 1.030; albumen, both by heat and nitric acid. Acidulated with a few drops of acetic acid, boiled, and allowed to stand, albumen $\frac{1}{10}$. Microscopic examination showed the presence of a large amount of uric acid and oxalate of lime, but was otherwise negative. Hunyadi janos water on rising, with alkalies before meals, a regulation of the diet, and exercise in the open air were prescribed. The urine continued to contain albumen in diminished amount for the three following days, when the record of the exam-

ination is as follows : React. neutral ; sp. gr. 1,020 ; no trace of albumen obtained either by heat or nitric acid. Microscopic examination negative. There was accompanying improvement in the general symptoms. During the past ten months the urine has been repeatedly examined (last date Dec. 1, 1881), but never at any time has it contained albumen ; uric acid and oxalate have also been constantly absent.

CASE 3.—Mr. B., student, aged 17. Has never had diphtheria. Had scarlet fever five years ago, without albuminuria. The urine was repeatedly examined by me during and subsequent to his illness, and at no time contained a trace of albumen. Has always enjoyed excellent health. Is a large eater. In Oct., 1880, while in apparent perfect health, on account of a remark that there was a sediment in his water, the urine was examined. The record is as follows : React. highly acid ; sp. gr. 1030 ; albumen, both by heat and nitric acid. Acidulated, boiled, and allowed to stand, albumen $\frac{1}{12}$. Microscopic examination : A large amount of uric acid and oxalate of lime present ; otherwise negative. A saline cathartic, alkalies, regulation of diet, and out-door exercise were prescribed. An examination of the water 36 hours later showed a normal sp. gr. and acidity, an entire absence of albumen, of uric acid, and oxalate of lime. The urine has been repeatedly examined since (last date Nov. 25, 1881), but albumen has never been found ; neither has it contained uric acid or oxalate of lime. The patient's health has continued excellent.

In the above cases the marked degree of the albuminuria, its very transient nature, its constant association with uric acid and oxalate of lime are noteworthy.

CASE 4.—Mrs. D., aged 22. Patient is of nervous temperament and lithæmic diathesis. Has never had diphtheria. Had scarlet fever, without any evidence of renal trouble during childhood. Has been under my observation for several years, during which time she has enjoyed excellent health.

The usual routine examination of the water was made on several occasions during this period, in trifling ailments, but albumen was never found. Late in the past autumn my advice was sought for the relief of morning headache and general lassitude. The patient also complained of moderate constipation, flatulence after meals, and a coated tongue. There was no fever or other

evidence of more than a temporary functional disturbance of health. For several weeks previous she had been living a very luxurious life, eating heartily and taking little exercise.

An examination of the urine, an after-breakfast specimen, showed a highly acid reaction, with a sp. gr. 1030; very considerable albumen both by heat and nitric acid, and oxalate of lime in large amount. An evening specimen of the same date resembled the morning's in all respects, with the exception of possessing a higher sp. gr., viz., 1032. Advice, similar to that which has been mentioned in the previous cases, was given and adopted, with the result of speedily relieving the subjective symptoms. Examination of the urine four days later showed a diminution in the acidity, lower sp. gr., and a mere trace of albumen in the evening specimen only. Uric acid was also found in the evening water alone and in small quantity. Examination of the urine on the fifth subsequent day: react. acid; sp. gr. 1022; no trace of albumen on very careful testing with both heat and nitric acid, in either a morning or evening specimen. Microscopic examination, negative. Two weeks later the examination was repeated. The record is as follows: After-breakfast specimen, react. acid; sp. gr. 1025; albumen absent, also uric acid and oxalate of lime. Evening specimen, react. highly acid; sp. gr. 1034; albumen, both by heat and nitric acid; urates precipitated by nitric acid; acidulated, boiled, and allowed to stand, albumen $\frac{1}{12}$. Microscopic examination: very numerous oxalate of lime crystals.

The patient had discontinued her medicine, and had taken little exercise during the previous week. She had, moreover, been suffering from much mental anxiety. Greater care in diet and more vigorous out-door exercise were urged and adopted, and on the following examination, Dec. 10th, a week later, the urine was found to be entirely normal. The points of especial interest in the above case are the transient character of the albuminuria, its invariable appearance and disappearance in the presence and absence of uric acid and oxalate of lime. The patient has not been under my observation for a sufficiently long period to express a positive opinion in regard to the absence of structural kidney change, but the symptoms certainly very closely resemble those observed in the group which I have described.

CASE 5.—Ethel M., aged 4 years. Has never had diphtheria or scarlet fever. The urine was examined on account of advice being sought for incontinence of water, the patient being in excellent health at the time. It was found to be markedly acid, of

high sp. gr., to contain albumen $\frac{1}{14}$, and uric acid with oxalate of lime in large amount. Two days later the urine was free from albumen and the above salts. The patient has been under my observation up to the present date, three years from the first examination; her health has been unusually good, and in frequent examinations (last date Nov. 12, 1881) the urine has been invariably normal.

CASE 6.—Ethel W., aged 9 years. Had scarlet fever without albuminuria when 4 years old. Has never had diphtheria. The patient has a neurotic family history. In July, 1880, had a convulsive attack, which was attributed to indigestion. Subsequent attacks demonstrated their epileptic nature. Under treatment further paroxysms have not occurred. The record of an examination of the water, Oct. 8, 1880, is as follows: Evening specimen, react. acid; sp. gr. 1031; distinct amount of albumen both by heat and nitric acid. Microscopic examination: a large amount of uric acid present.

An examination of an after-breakfast specimen of the same date gave similar results, differing only in having a lower sp. gr. Examination a few days later, morning specimen: react. acid; sp. gr. 1014; free from albumen; microscopic examination, negative. Evening water, same date: react. acid; sp. gr. 1026; *trace* of albumen, with *few* uric acid crystals present. On the following examination, although the sp. gr. of the urine was 1033, it was wholly free from uric acid and oxalate of lime, and did not contain a trace of albumen.

Very frequent examinations have been made up to the present date, and the records show the invariable absence of any trace of albumen; also the absence of uric acid and oxalate of lime. It may be mentioned that the diet has been most strictly regulated during this period, and careful attention given to all other hygienic measures.

CASE 7.—Florence T., aged 4 years. The patient has a lithæmic diathesis. Has never had diphtheria or scarlet fever. During the winter of 1880 advice was sought for an attack of urticaria. The tongue was coated, the bowels disordered, and the cutaneous symptoms very pronounced. There was no fever. Examination of the urine: react. acid; sp. gr. not recorded; a considerable amount of albumen both by heat and nitric acid; urates precipitated by the addition of acid; microscopic examination, amorphous urates.

The following examinations gave entirely negative results. The

child has continued to be in excellent health up to the present date.

Urticaria is now universally ranked among the neuroses, and it is a neurosis in which the vaso-motor system is pre-eminently affected.

Moreover, a temporary lithæmic state is recognized as a frequent factor in its genesis. The above case may therefore not unreasonably be regarded as lending support to the theory of transient albuminuria occurring in apparent health, which has been suggested in this paper. I possess the notes of two other cases of temporary albuminuria in children of 4 and 5 years of age respectively, associated with transient lithæmia, but as they closely resemble the cases already narrated, their recital is unnecessary. I have at present under observation four additional cases in adolescents, but as they have only recently come under my notice, I do not feel warranted as yet in including them among the above.

Inasmuch as it has been wisely said "that much of that which each man thinks that he observes is but a part of himself," it may be mentioned that Dr. Draper kindly consented, at my request, to repeat the examination of the urine in many of the cases which I have recorded, with the result of corroborating my observations.

The presence of uric acid, either in the crystalline or amorphous form, even in large quantities, in a concentrated and highly acid urine, cannot be accepted as a demonstration of its excessive elimination from the body. Oxalate of lime, however, is present in the normal urine only in minute quantity (Schultzen¹) if at all (Neubauer²); urine, therefore, found to contain a large amount of oxalate of lime within two or three hours after its passage, may fairly

¹ *Archiv f. Anat. und Physiol.*, 1868, p. 719, *et seq.*

² "Analysis of the Urine," Neubauer and Vogel, p. 168.

be judged to indicate an excessive elimination of this imperfectly oxidized nitrogenous matter.

In my own cases the urine in several instances was examined two hours after being passed, and showed the presence of a large quantity of oxalate of lime crystals alone.

Moreover, in the majority of all the cases the constitutional symptoms were of a character usually present in lithæmic patients.

The very constant association of imperfectly oxidized nitrogenous matter in the urine and albuminuria, especially in Dr. Moxon's and my own cases, cannot reasonably, it seems to me, be regarded as a simple coincidence; in the latter observations we find an additional argument against such an hypothesis in the disappearance of the albuminuria *pari passu* with the lithæmia. Dr. Murchison¹ maintained that such an association, at a later period in life, not only was to be regarded in the light of a causal relation, but he also agreed with Dr. George Johnson in believing that "renal degeneration may be a consequence of the long-continued elimination of products of faulty digestion through the kidneys."² That temporary albuminuria in children and adolescents in apparent health is invariably dependent upon a transient oxaluria or lithuria, I do not maintain. The observations of Leube, Ultzmann, Fuerbringer,³ and others would seem to show that prolonged and vigorous physical exercise is capable of occasionally producing, though in an as yet unsatisfactorily explained manner, a slight transient albuminuria in presumably healthy persons. I have not referred to the theory of chemical or "food albuminuria" in explanation of the condition which is the subject of this paper, for the reason that careful investigations and experiments leave it extremely doubtful whether albuminuria (the presence in

¹ "Clinical Lectures on Diseases of the Liver," Amer. edition, pp. 572, 573.

² *Brit. Med. Journ.*, 1872, vol. I, pp. 161-191.

³ *Loc. cit.*

the urine of a body coagulated by heat or precipitated by neutralization—Saundby) is ever due to the transudation of a modified or more easily diffusible form of albumen. Dr. Saundby has shown, by a series of very interesting observations and experiments,¹ that the several tests, viz., an increase of albumen after food, a greater diffusibility of such albumen (Parkes, Pavy), and a difference in the coagulation temperature (Lauder Brunton and D'Arcy Power), which have been used by different investigators in their attempt to prove this point, are not to be relied upon.

In conclusion, my observations would seem to show that temporary albuminuria, as it occurs in children and adolescents in apparent health, may be traced in a large number of instances to a transient oxaluria or lithuria, and I would suggest that the sequence of events in the causation of the albuminuria is as follows:

1. The temporary presence of a large amount of imperfectly oxidized matter in the circulation.
2. A disturbance of the general nervous system, in which the vaso-motor system of the kidney shares, or one confined to the vaso-motor system of the kidney in its elimination of these products of a faulty digestion.
3. A transient dilatation of the blood-vessels of the kidney and a retardation of the blood current in the glomerular vessels, with a consequent possible alteration in the functions of the glomerular epithelium, also of a temporary nature.

¹ *Brit. Med. Journ.*, vol. I, 1880, p. 841; *Birmingham Med. Review*, July, 1879.

NOTE ON A CASE OF LOCALIZED CEREBRAL ATROPHY.

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THE physiological experiments of Hitzig, Ferrier, Carville and Duret, Munk, and many others, have tended to show that in the cerebral cortex there are more or less localized motor and sensory centres; a large quantity of pathological material has been collected in support of these physiological deductions by Charcot and his pupils Ferrier, Seguin, De Boyer, and many others.

The largest part of this material collected is in support of the doctrine of localization of motor centres, and up to this time our knowledge of the sensory centres, physiologically and pathologically, is quite limited.

With regard to these sensory centres Ferrier says: "When the lesion is accurately circumscribed in the angular gyrus the loss of vision is the only effect observed, all the other senses and the powers of voluntary motion remaining unaffected."

He then details an experiment in which the angular gyrus was injured, and says: "The animal retained complete muscular power and every other form of sensation, except sight, the conditions as regards which being repeatedly tested in various ways." And he further says: "Destruction of the angular gyrus on one side causes blindness in the opposite eye; the loss of vision is complete but

is not permanent if the angular gyrus of the opposite hemisphere remains intact, compensation rapidly taking place, so that vision is again possible with either eye as before. On destruction of the angular gyrus in both hemispheres, however, the loss of vision is complete and permanent, so long at least as it is possible to maintain the animal under observation." Ferrier, therefore, concludes that the centre for vision is in the angular gyrus. After detailing some experimental injuries of the superior temporo-sphenoidal convolution, he says :

"When the lesion was established bilaterally so as to cause destruction of the temporo-sphenoidal convolution on both sides, along with certain other effects not depending on localized injury of this convolution, the animal, though fully conscious and on the alert to every thing attracting sight, failed to respond to auditory stimuli usually exciting active reaction and attention."

He, therefore, places the auditory centre in the superior temporo-sphenoidal convolution.

Munk finds that extirpation of a particular part of the occipital lobe in dogs causes psychic blindness; that is, the memory for visual impressions previously received is abolished, and after extirpation of the two occipital lobes in monkeys they become completely blind, they are unable "to see any thing."

Ferrier¹ gives no pathological cases in support of his physiological localization of the centres of hearing and vision.

De Boyer² says, in regard to this subject, that "the pathological facts yet reported are not sufficiently numerous, and are too dissimilar in character for any deductions to be made from them."

¹ Ferrier: *The Localization of Cerebral Disease*. New York, 1879, lecture iii.

² De Boyer: *Etudes Cliniques sur les Lésions Corticales des Hémisphères Cérébraux*.

The case now reported is not strong evidence of this localization of centres of hearing and vision, but is remarkable in that the localized atrophy found occupies the region in both hemispheres in which these centres have been located by Ferrier; the case also has some interest in regard to the localization of motor function in the cortex.

A case has just been reported by Dr. Chauffard¹ of what he entitles cerebral blindness and deafness.

Male, aged 44, without serious disease up to the beginning of this illness; two or three months ago began to have œdema of lower extremities and abdomen; at present he has marked œdema of lower extremities, and ascites; urine scanty and contains albumen; face cyanosed; no valvular disease of heart discovered. Oct. 10th, in the evening, without preceding symptoms and without loss of consciousness, the patient loses at once his speech and the use of his intellectual faculties, and the next morning we find him in the most curious cerebral state; remains seated on his bed in the posture habitual to him, and the only one which the size of his abdomen permits; supported on his two elbows, he remains immobile, eyes fixed, lost in vague and apparently profound meditation; he is spoken to, shaken by the shoulder, the hand is brought quickly before his eyes, he sees nothing, he hears nothing, he appears to be unconscious of what is going on around him; if he is spoken to loudly and sharply he, nevertheless, turns his head to the side from which comes the sound, but without permitting the belief that he comprehends what is said to him,—that the auditory impression which he has vaguely received has awakened in him an idea or a memory.

The same for vision: the pupils are partly dilated and immobile, the eye vague, it follows not the objects which pass in the visual field, and, nevertheless, the patient is not properly speaking amaurotic.

Besides these curious troubles the patient still preserves the use of his motor functions.

Nov. 12th.—Same state of absolute cerebral unconsciousness; the patient appears to be a perfect stranger to all that surrounds him. He remains silent, muttering, half audibly, incoherent and

¹ Chauffard: Note sur un Cas de Cécité et Surdité Cérébrales; *Revue de Médecine*, No. 11, November, 1881.

unintelligible words, or repeating, ten times an hour, mechanically and with the same monotonous intonation, "I am better, I am better."

Ophthalmoscopic examination shows no lesion at fundus oculi. Patient died Nov. 13th, and autopsy shows integrity of meninges on both sides. Brain normal, except left hemisphere, and it contains one lesion of considerable extent, visible at first sight and perfectly isolated. It is a focus of red softening of circular form, about the size of a five-franc piece, and occupies *le lobule du pli courbe et le pli courbe*. Above, it reaches the interparietal fissure without passing it; below, it extends over the superior extremity of the temporo-sphenoidal fissure (*scissure parallèle*) and the two convolutions which limit it, first and second temporal. This lesion occupies the entire thickness of the gray substance of the convolutions, impinging even very lightly on the white matter.

PERSONAL CASE.—M. R., aged 34, admitted to asylum Sept. 25, 1879; married; has had four children, the last fifteen months ago. The husband of the patient is a very ignorant and drunken man, and can give no history of his wife's condition. The following is obtained from her sister: Two months before admission she complained of being unable to use her right upper extremity. She could not lift any thing with it; thinks she had no trouble with the right lower extremity. Soon after she had a sudden attack, characterized by sudden loss of consciousness, inability to speak, and loss of hearing. They did not notice at this time that she had any loss of vision. She remained unconscious two weeks (?), after which time she was able to get out of bed, and then began to talk, but she would talk sensibly only for a few minutes; then she would ramble off and become incoherent. It would appear that at this time patient must have had trouble with her vision, although the friends are quite indefinite about it, and she evidently at this time had marked mental disturbance. Patient told her sister that the sudden attack was preceded by a pain in right ear, then pain in left ear and side of head, and she knew no more. She has had no convulsion since the first attack up to this time. It is said that she was paralyzed on the right side after this attack, but it soon passed off.

The certificates committing her, relate that she says some men are trying to kill her, and she screams without cause, and is at times violent. On admission patient is very stupid; does not answer any questions, evidently does not hear them; is quite deaf; pupils slightly dilated and regular. She is in poor physical condition:

scars on face, evidently quite blind, bowels constipated, digestion poor, pulse feeble.

Oct. . . .—It is noted that she soils her clothing and bedding, and goes groping about the hall, talking disconnectedly and at times screaming out.

Oct. 18th.—Had a severe epileptiform fit, which lasted about three hours ; the muscular spasm began on right side, and became general ; cerebral tumor is suspected.

Oct. 21st.—Was very stupid after the convulsion ; to-day has improved, but is quite restless ; tosses about and screams aloud as if in pain.

Nov. 6th.—Still remains very heavy and stupid ; sleeps the greater part of the time.

Nov. 21st.—Has been in bed without special change most of the time since last note ; seldom sits up ; to-day had a convulsion, with marked spasm of left side of face ; stertorous breathing ; limbs relaxed.

Dec. 2d.—Slowly recovering, but is still very stupid from last fit.

Dec. 11th.—Repeated tests of tactile sensibility and smell show them to be intact, and repeated testing shows her to be absolutely deaf to all noises. All the other cranial nerves appear normal in function.

Dec. 16th.—Walks about the hall, using disconnected and unintelligible words ; at times speaking very loudly, and screaming.

Dec. 22d.—From the way in which she uses her right hand it is inferred that she has slight paresis of that extremity ; it is impossible to apply the dynamometer, as she is so very stupid, blind, deaf, and screams and resists when interfered with ; for the same reason no ophthalmoscopic examination has been obtained ; she presses her hands to her head as if in pain ; at night is very noisy.

Jan. 31, 1880.—Had a severe convulsion, which lasted from 10 A. M. to 5 P. M. ; face very much congested ; pupils dilated ; profuse salivation ; muscular spasm, most marked on right side. At 12 M. (midnight) was quieter ; T. 96° in axilla ; at 2 A. M., T. 98° F.

Feb. 1st.—At 10 A. M., T. $100\frac{2}{3}^{\circ}$; P. 100 ; 7 P. M., T. 101° ; P. 108 ; lies all drawn up in bed.

Feb. 2d.—10 A. M., T. 99° ; P. 80 ; 7 P. M., T. $98\frac{1}{2}^{\circ}$; P. 76 ; muscles of right arm quite firmly contracted.

Feb. 6th.—Patient uses right arm a little this afternoon.

Feb. 14th.—Is up again ; walks about ; shows no sign of paresis anywhere.

Oct., 1880.—She has become quite noisy, talking and screaming unintelligibly night and day ; is growing thinner and more stupid, if that were possible.

Patient remains in about the same condition until Sept., 1881, when she develops a low form of pneumonia, and dies Sept. 19, 1881.

Post mortem Examination.—Only the head examined ; dura to naked eye appears normal ; nothing special about skull and sinuses ; slight meningitis everywhere, but it is not marked. There is a remarkable atrophy of the superior and inferior parietal convolutions on both hemispheres ; the atrophy is so marked and localized that it presents a striking contrast with the other portions of the brain. The cranial nerves present a normal appearance to the naked eye ; the atrophied convolutions are quite firm to the touch ; there is no atheroma of vessels.

The brain is hardened in bichromate of potassa, further examination of which shows that the upper part of the ascending parietal on the left side is very slightly atrophied, and the same convolution on the right side is also very slightly atrophied at its lower portion. This atrophy is in a very limited part of the convolutions.

To give some idea of the degree of atrophy, we will take as a comparison the first temporo-sphenoidal convolution in a brain hardened in bichromate of potassa and without special wasting. A rough measurement shows it to be about $\frac{3}{4}$ to 1 inch broad, and in the same convolution of our patient it is $\frac{1}{4}$ inch, and less in some places.

During the life of this patient it was at first supposed that she might have cerebral tumor, but this did not explain the association of absolute blindness and deafness without the involvement of other cranial nerves.

It was suggested by Dr. Seguin that it was possibly meningitis, which at first appeared to be the true pathological

diagnosis, but on consideration it appeared improbable that a meningitis should disturb so completely the functions of the auditory and optic areas and leave all the others intact, and in the absence of a more complete history, it appeared impossible to arrive at a satisfactory pathological diagnosis. On *post mortem* examination the atrophy was found so strictly localized to the convolutions in which have been located the centres of vision and hearing by Ferrier, that the question at once arose, is it possible that this is a case of absolute blindness and deafness due to cortical lesion? and it appeared a very fascinating view of the case.

Microscopic examination was made of the atrophied convolutions, and it was found that the gray matter had entirely disappeared, leaving the very outer layer, to which the pia was attached, and the white matter below, so that there was really a cavity, or space, between the two, formed at the expense of the gray matter. Microscopic examination showed this outer layer to be composed of dense connective tissue filled with nuclei.

The white matter under it contained an enormous quantity of nuclei of all sizes; the perivascular spaces were very much dilated; and from the vessels to the wall of the perivascular space many bands or fibres of connective tissue ran, and in which were entangled hematoïdin crystals; the walls of the perivascular spaces and the tissue immediately around them were the seat of a large quantity of closely placed nuclei; the blood-vessels appeared normal.

Examination of the optic nerve microscopically shows a more or less general increase of the connective-tissue septa running between the nerve bundles; the nerve fibres themselves are atrophied; and there are numerous small areas of disintegration of nerve fibres, the spaces being filled with a colloid-looking (?) material, which is faintly stained with hematoxylin: this is a similar appearance to that seen

in spinal cords which have undergone degenerative processes.

What relation do these pathological conditions bear to the symptomatology in this case?

The atrophy of the angular gyri and the first temporo-sphenoidal convolutions, the supposed centres for vision and hearing, tempts one to assert that in this is to be found



FIG. 1.—Distribution of cortical atrophy shown upon an Ecker's diagram: right hemisphere.

the explanation of the blindness and deafness, the association of which, without involvement of other nerves, was so difficult of explanation during life; but the microscopic examination of the optic nerves deters me from making this as a positive statement, as a second question arises: Are the appearances found in the optic nerves due to primary lesion in them, or are they degenerative, and due to the

destruction of the cortical visual centre? It appears to me *possible* that these changes are degenerative, and due to the lesion of the cortex, but no more definite answer can be given.

It has been noted that the patient, from her movements, appeared to have some paresis of the right upper extremity, which passed off; and at another time it is noted that she



FIG. 2.—Distribution of cortical atrophy on the left hemisphere.

had marked rigidity for several days of the right arm, which also passed off. These passing motor disturbances appear to be explained by the proximity of the lesions found to the motor district—the ascending frontal and parietal convolutions,—especially as there is a small place on each hemisphere where the ascending parietal convolution is visibly wasted. It is of interest to note, however, that these motor

difficulties were passing, not permanent; and this shows that lesions, in the brain particularly, may be so slight, and at a particular moment they may cause more or less grave motor disorders—paresis and convulsions even,—which are passing; and that the lesions must be of considerable extent and severity to cause permanent motor disorders.

The absence of accurate information as to the state of vision at the onset of the attack in our patient is to be regretted; the sudden loss of hearing appears to have been made out pretty clearly. This case resembles in many respects the one reported by Chauffard: in his case the lesion was in one hemisphere, and there were absolute blindness and deafness; in our case the lesion was in both hemispheres, and in that respect more in keeping with Ferrier's physiological experiments; the mental condition of our patient appears to have been about the same as in Chauffard's.

EDITORIAL DEPARTMENT.

SPECIALISM IN MEDICINE.

We propose to consider briefly, but critically, the following proposition, which, though not distinctly formulated, is, as it were, held in solution in many others now current, and may be easily precipitated from them.

At the present day medical science has expanded to such an extent that its intelligent cultivation as a whole by any one person has become impossible. The practice of medicine, therefore, to the extent to which it may reach any really high standard of excellence, must henceforth be carried on exclusively by specialists.¹

Thus, the physician, who should, in chimerical imitation of Lord Bacon, propose to "take all (medical) knowledge for his portion," must, on this theory, be consigned to a limbo of worn-out inanities. Nevertheless, the most useful functions of specialists are still exercised with tacit reference to the intelligent practitioner, who is compelled, not indeed to know all about all medicine, but to hold the key of admission to any of its branches, of which, at any moment, he may have practical need.

Thus, specialists are justly expected to become the depositories of special literature, and to so sift, handle, classify, and arrange

¹ * * * "The fact, the hard and undeniable fact, that all intelligent and scientific physicians are quasi-specialists, and must be. In the present development of medical science there is no alternative; a physician must be a quasi-specialist, or possess a universal knowledge of a superficial, mostly booky kind,—a knowledge wholly insufficient to insure intelligent or successful practice."—E. C. Seguin, these ARCHIVES, April, 1881, p. 186.

this, that it become accessible to, and utilizable by the general practitioner. By reiterated experience, they are expected to acquire an exceptional familiarity with certain types of disease, so as to be better able to decide in rare, obscure, or unusually difficult cases, when the physician shall call them in. By continued application they may tend to indefinite improvement in the technique of diagnosis and of treatment. Finally, in regard to the state of medical knowledge on any given question at a given moment, they may furnish the standards with which the knowledge and practice of the general physician must constantly be compared and tested. Thus, specialism is largely useful in furnishing the *exact* material with which the general physician may make his practical combinations. In his absence, and from the languid interest which specialists profess in each other's departments, this combination would often not be effected. But the problem offered by a sick person is always a problem of combination. The practical specialist does not analyze, but roughly divides this problem according to considerations frequently artificial. The scientific specialist abstracts phenomena completely; studies separately, anatomical, physiological, chemical, pathological conditions. It is the ideal business of the physician to take conditions which science has abstracted for the purpose of thought, and to recombine them for the purposes of life. In the absence of the physician there would be no one to do this; with every new deterioration of the ideal character of the general physician, this work of combination is less and less well done. As a consequence, every sick person who can pay for it begins to expect to divide up his body among a cluster of "eminent specialists" before any positive diagnosis of his case can be reached.

Notwithstanding the inconvenience and expense of this procedure, it tends to gain in popularity on account of the simplicity and apparent common-sense of its theory. The laity are very ready to infer not only that specialism is good, but that the more of it the better. If the physician who treats six diseases is necessarily superior to him who is willing to manage sixty, then he who confines himself to one must be the best of all. Hence the

popularity of the pile doctor, and the cancer doctor, *et hoc genus omne*.

The great principle of unity in diversity, whose research is the problem of philosophy, is also the animating principle of philosophical medicine. But this cannot be appreciated by persons who are neither physicians nor philosophers.

The complete theory of practical specialism admits that a man may be a shining light in a subject "which interests him," yet a perfect idiot in another of equal importance to the patient. Now, the initial problem of diagnosis is the decision of the department to which the case belongs; and, on the above theory, the fate of the patient must be a matter of chance. If his case happen to fall on the competent side of the doctor he consults, well and good; but if not, it must fail of recognition. No fixed value can be attached to any symptom, when it is remembered that the lines of disease intersect each other in every direction.

Thus, does a young girl fall into a melancholy? The question would arise: Shall she be at once entrusted to the gynecologist on the suspicion of uterine disease, or to a hæmatologist for chloro-anæmia, or to the superintendent of an asylum as a case of incipient insanity, or to a friend of the family to bring about a thwarted project of marriage? If a woman has a pain in her back, how many physicians must be consulted before deciding whether this be due to muscular denutrition, or to uterine displacement, or to chronic nephritis, or incipient myelitis, or to commencing caries of the vertebra, or merely to hysteria? When a typhoid fever simulates general tuberculosis, or the reverse, should the diagnosis be made by the heart and lung specialist, or by the fever doctor? When a man falls down in an apoplexy, does his case belong to the neurologist, or to the specialist in diseases of the heart whence an embolus may have been carried, or to the practitioner devoted to gout and atheroma? Shall a children's doctor decline to perform an urgent tracheotomy because he is not a surgeon? or shall a physician tolerate irreparable delay in reducing a dislocation for the same reason?¹

¹ We have within a few weeks seen two cases of irreparable injury caused by

It is sometimes said that the conscientious specialist will be sufficiently trained in general pathology to recognize when a subject lies beyond his domain, and he will then, "in justice to his patient," hand him over to one of his own "eminent colleagues."

Dr. Barnes, who, of all gynecological specialists, most frequently deprecates specialism, thus illustrates the case: "A woman comes to him complaining of pruritus. Much to her astonishment, he examines her urine, because he retains enough knowledge of general pathology to know that pruritus may indicate diabetes. Finding sugar, he at once resigns the case and sends her elsewhere." This illustration represents a class of cases which do often occur, and where the specialist is really both competent and conscientious the case may be managed without further inconvenience to the patient than that of a double consultation. But—and this is a practical inconvenience of perhaps a low order for mention here—there is certainly no more, but rather less, guarantee for the honor of a specialist than of a general practitioner. The last is expected to take charge of the patient whatever may prove to be the matter with him. His interest, therefore, in ascertaining the exact state of things is identical with that of the patient. But the specialist knows he will only be entrusted with the case if he can prove that it falls within the limits of his own specialty. He is therefore always under a strong temptation to "make out a case," and for this purpose, if necessary, to rather avoid than to seek close scrutiny of the surroundings.

We hasten to recognize the fact that there are many specialists of honor as high and unsullied as could be claimed for the most upright physician. But we think the existence of the special temptation we have referred to can hardly be doubted, nor that this temptation is by no means always resisted. Apart from this purely practical consideration, it is to be remembered that such definite grounds of classification are more often absent than present; the specialist confronts the theoretical difficulty of not being quite sure what he is to exclude.

just this fact, and by the prolonged application of poultices instead of prompt operative interference.

Another important inconvenience in the tendency to universal specialism is that the beginnings of disease are so often likely to escape detection. To consult a specialist, the patient will first wait until he is pretty sure he has the specialist's disease ; thus, he must wait until this is rather well developed. Thus, too often no attempt is made to treat a chronic disease until it has become almost incurable, nor to make the precise diagnosis of an acute disorder until it has nearly killed the patient.

But the collapse into inefficiency of a general practitioner is not an adequate basis upon which to develop an accomplished specialist. Instead of either the one or the other, we obtain a confused, vague, cheerfully optimistic "family doctor," who relieves himself of responsibility for one organ in his patient's body after another on the ground that it belongs to some "specialist," who, as long as symptoms are not importunate, declares that they will "pass away of themselves,"—instinctively dreading the recognition of their importance as the signal for a surrender of the case. Thus, epitheliomas are allowed to extend until they are ineradicable, and chronic pneumonia to eat out caverns in lung tissue unsuspected, and the child to limp from habit into a suppurating coxitis, and the melancholic to commit suicide while sent on a journey for change of scene.

In addition to the functions which may be unquestionably fulfilled by specialists with great advantage to the community at large, other claims are often advanced of, we believe, less validity. Thus, it is said :

1. That to specialists alone, or chiefly, is due not only the improvement of technique, but the discovery of the fundamental ideas which change the face of science.

2. That specialists are habitually engaged in life-long researches in the subjects of their specialty.

3. That, thus, the patients of a specialist must profit much more by his intellectual activity than can the patients of a general practitioner by his.

4. That, whereas a general practitioner can only have at best a partial acquaintance with the many diseases he treats, the special-

ist, in virtue of his wise limitation of observation, can know all about his.

5. Finally, that the establishment of specialties alone permits the accumulation of clinical material in definite and available masses.

The first claim might be contested *a priori* from the consideration of the evident necessities of the case. No idea in a specialty can be as fundamental or as original as that on which the specialty is founded, and this evidently must have been suggested by a non-specialist. Laennec was not a specialist when he practically discovered the principles of auscultation; his prolonged special application afterward was devoted to the consolidation and simplification and detailed establishment of his theory. Helmholtz was no oculist when he invented the ophthalmoscope; even his treatise on optics was written later. Czermak was not a specialist when he invented the laryngoscope. Orthopedics, perhaps, dates its modern impulse from the researches in locomotion of the brothers Weber, who were physiologists. The principle of counter-irritation in joint diseases was established by Pott, a general surgeon of London; the still more important principle of rest was elaborated by Bonnet, a general surgeon of Lyon. The effective introduction into orthopedic surgery of resection was made by Sayre before he became an orthopedist. In gynecology the capital operation of ovariectomy was initiated, as is well known, by McDowell, a general surgeon, having been originally suggested by Hunter, than whom none of the great physicians of the eighteenth century was less of a specialist. It was the great surgeon Velpeau, and the author of a treatise on neuralgia, Valleix, who first called attention to uterine flexions and suggested pessaries. Dr. Sims had hardly become a specialist when he invented his speculum and contrived his operation for vesico-vaginal fistula, achievements which his long career has never enabled him to excel.

Modern dermatology is based upon anatomical researches, which may be, and often are, carried on by histologists who do not practise medicine at all,—hence could not be called practising specialists. The clinical researches of the French school, being con-

ducted according to the theory of diathesis, were not and could not be made by physicians limited in clinical observations of skin diseases. The theory may be discarded ; but the results of the impulse given under its influence remain. In neurology clinical specialism was first suggested by anatomy, and later by physiology. In no practical specialty is modern clinical observation kept more closely to these two fundamental sciences than in this. The principal facts and ideas have come from anatomists or physiologists, or from non-specialists, who have also furnished the chief clinical groupings. Bell's discovery of the double function of the roots of nerves was made in his capacity of anatomist ; his discovery of external facial paralysis, in his capacity of general practitioner. Marshall Hall, Brodie, Abercrombie, Calmeil—even Broussais, with his "*De l' Irritation et de la Folie*,"—and a host of others, who were the early pioneers in this century in the study of nervous diseases, were not specialists, since it was indeed at that time not possible to be one. Nevertheless, many of their observations remain of permanent and fundamental value. The most eminent physiologists, who have contributed to knowledge of nervous diseases far more than have simple clinicians, have not been specialists in the physiology of the nervous system. Magendie, who divides with Bell the honor of the discoveries in the spinal roots of nerves, wrote two volumes on the "*Physics of the Animal Organism*." Bernard is as distinguished for his composite researches in diabetes (to go no further) as for those on the vaso-motor system. Schiff, who distinguished the paths in the cord for different sensory impressions, has written a treatise on digestion. Neither Türck nor Bouchard were practical specialists when they established the fact of descending degenerations ; nor was Waller when he made the famous experiment which has served to explain these morbid processes. Brown-Séquard's researches in epilepsy were made at the very beginning of his career, and not when he had become a specialist. The clinical groups of locomotor ataxia and pseudo-hypertrophic paralysis were established by Duchenne, whose specialty was not nervous diseases, but faradic electricity, and originally, in its application to orthopedics. Ex-

ophthalmic goitre has been discovered by Basedow, a sagacious general practitioner; and the same is true of Addison's disease. Gubler, the first to point out crossed paralysis, was never a specialist; indeed, his essay on the hepatic lesions of hereditary syphilis is as famous as any that he has written. Sir William Gull's and Stanley's observations on paraplegia from renal calculus initiated research into "reflex paraplegia." No one could suppose them to be specialists.

Another class of examples is offered by writers who had become specially identified with neurological practice before publishing the treatises now recognized as authoritative, yet who, before this, had achieved distinction in other directions. Thus, Griesinger's now classical work on psychiatry was preceded by an only less famous treatise on infectious diseases. Leyden, before writing two volumes on diseases of the spinal cord, had published a valuable monograph on icterus. Nothnagel's admirable clinical contributions to the problem of cerebral localization, and his less admirable experiments on the brain, cannot efface recollection of his hand-book of therapeutics—on the whole, the most valuable extant on the subject. Charcot began his studies in neurology by general studies on the diseases of old age. He was stimulated by the practice of no specialty, but simply utilized the neglected pathological materials accumulating in oblivion at the Salpêtrière. Only recently, moreover, Charcot has published a series of lectures on the pathology of the liver and of the kidney; and his description and analysis of the lesions of broncho-pneumonia have thrown new light on a subject supposed to have become hackneyed.

These examples, selected at random, do not of course exclude the clinical discoveries or inventions which have been made by practising specialists, and in a manner which indicates that they were the direct outgrowth of their special clinical experience. In neurology, Westphal's discovery of the tendon reflex symptom; in gynecology, Emmet's operation for lacerated cervix, are typical examples of this class. The fact that Hitzig, whose discoveries on the motor irritability of the cortex have had such an enormous

influence, has been for a long time the superintendent of an insane asylum, is not an example of the influence of practical specialism. His researches were purely physiological, and were suggested by physiological considerations, which clinical observations might confirm, but did not suffice to originate.

We think the cases quoted are sufficient to demonstrate that indefinite repetition of clinical experience is never of itself sufficient to suggest new ideas; that a life-long specialism in no wise predisposes to discoveries, and still less is essential to their achievement; that in a large number of cases, if not the majority, the consecration to a specialty has followed, and not preceded, the discovery which has achieved the reputation of the specialist, and has fascinated him, perhaps for ever, with the subject. But it is always genius which invents; special application can only improve; it then remains for culture to appropriate.

Our limits compel us to be brief with the three remaining propositions. In regard to the second claim, namely, the life-long researches supposed to be carried on by practising specialists, we would call attention to a fact usually overlooked. It is that for every mind, in regard to every subject it studies, there exists a saturation point of suggestiveness, which is not exceeded by enforced prolongations of attention. It is very useful for a person to pursue a subject, so long as it continues to yield him ideas; very useful to practise a technique, until it be sufficiently mastered to meet all difficulties of execution. But afterward there remains no intellectual advantage in persistent adherence to the same line of thought. There are personal, often pecuniary advantages; there is profit gained from an acquired reputation and previous labors. But this, however legitimate, is a very different thing from continued progress in science, or indefinite improvement in care-taking of patients, such as is generally assumed.

Again, the practical specialist does not, fortunately, often select only one disease, but one organ, or presumably associated group of organs. Now cases of the same disease in different organs are apt to present many more points of resemblance than do cases of different diseases in the same organ. There is much more analogy

between uterine cancer and epithelioma of the lip than between uterine cancer and uterine flexions. The study of the pelvic curves throws no light on embryology, although both subjects are assigned to the obstetrician. Uræmic peritonitis is better understood by study of septic peritonitis than of renal calculus. Epilepsy has much less resemblance to the systemic forms of myelitis than to the eclampsia induced by acute hemorrhages, and so on.

Practical specialism only enforces attention to clinical observation : analysis of this, on the basis of any special science, is as optional with the specialist as with the general practitioner, and as liable to be neglected. Many good specialists are purely clinicians ; many others, really distinguished in some branch of science connected with special disease, are quite innocent of others. Perhaps from few experts in consultation would we expect familiarity with such a monograph as Bert's on respiration, or with the complex laws on diffusion of gases. It would not be difficult to name neurologists distinguished in experimentation, but who have never mounted a section of nerve tissue for the microscope. It would not be impossible to cite skilful surgeons, most ingenious in mechanical contrivance, who are unaware of the pathological anatomy of the tissues they divide or remove.

Great as are the difficulties arising from the great increase in the mass of knowledge, there are many palliations. The perfected machinery for sifting, analyzing, classifying, and sorting this knowledge, renders it ten times as accessible and comprehensible as was formerly one tenth part as much. Many general principles have been established, which link together, in lucid unity, hosts of details, once unconnected, unintelligible, and hence most difficult to remember. The classical body of doctrine in medicine, whose possession is essential to the practice of medicine (*secundum artem*), is really more accessible to-day than at epochs when some narrow system professed to crush it into a portable nutshell. Finally, the advance of science and of scientific method exacts, that who would claim to contribute to further progress must concentrate himself much within the limits of any conventional specialty. No one disease, no one organ may be compassed by a

single observer : happy he who may, by laborious research, contribute to the solid establishment of a single detail of the truth. For such work it is, theoretically at least, as easy for the general, as for the special physician to withdraw a certain portion of his attention from practice. Neither can hope that his research can benefit more than a small proportion, if any, of his own patients. The one must, as much as the other, depend on the collaboration and unconscious coöperation of a thousand workers. For both, are needed not only clinical observations, but the mental ability to utilize observations,—a mental training in the art of handling large masses of ideas. For both, if we may judge from European examples, the personal experience to be gained in private practice is insufficient; to both, should classified hospitals be open as the true field for pathological study.

NEW BOOKS AND INSTRUMENTS.

Transactions of the American Gynecological Society.
Volume v. For the year 1880. Boston: Houghton, Mifflin & Co., the Riverside Press, Cambridge, 1881. 8vo, pp. 470.

This handsome volume, which typography, paper, and binding combine to render a pleasure to the eye, contains the year's record of an association which has already obtained a world-wide repute. Thanks to the achievements and incomparable energy of a few of its leading members, American gynecology stands to-day confessedly unrivalled as a surgical specialty. No association or society represents it except the one named in the title to this volume, nor has it any other means of bringing its views before the public. Hence, those who look to this body for instruction, new methods, and encouragement have a right to hope that their aspirations will be gratified.

Does the volume before us meet such expectations? An examination of its contents will furnish the answer.

The volume contains :

1. List of officers for 1880.
2. List of honorary fellows.
3. List of active fellows.
4. Minutes of the proceedings of the fifth annual meeting.
5. Papers read at the fifth annual meeting, consisting of the annual address by the President, and fourteen papers on various obstetric and gynecological subjects.
6. Three indices, elaborately and carefully prepared :
 - (a) Of obstetric and gynecological journals.
 - (b) Of obstetric and gynecological societies.
 - (c) Of gynecological and obstetric literature for the year 1879.

The President, Dr. Sims, deals in his annual address chiefly

with the progress of the Society, or rather its lack of progress, in extending its membership; and proposes certain modifications of the Constitution and By-laws, whereby he hopes that such defects will be overcome. As this subject concerns only the members, comment here would be out of place, although it may well be questioned whether any medical association could ever become popular with the profession, hedged in by the restrictions which Dr. Sims describes. To those who know him it is needless to say that his suggestions are urged with all the point and appositeness of illustration which characterize their distinguished author.

Of the papers that follow, the first is by Dr. Robert Battey, on the "proper field" for the special procedure of oöphorectomy devised by him, and which has rendered his name famous. As the points are not accurately formulated, although the paper is readable and interesting, no synopsis of it will be offered, but the reader referred to the original.

Two papers, which call for no special comment, follow—one by Dr. Engelmann, of St. Louis, upon Battey's operation in cases of anterior displacement of the ovary; the second on the "successful extirpation of an encephaloid kidney." Nephrectomy is, however, in this country so rare in comparison with European experience—it is said to have been performed only three times in this city—that every well-marked case is worthy of careful record.

Next in order is a paper by Dr. A. Reeves Jackson, of Chicago, on "Uterine massage," in which minute directions are given as to the application of this method for the relief of chronic subinvolution of the womb, or hyperplasia, or uterine hypertrophy. The frequent association of perimetritis with this condition lessens greatly the class of cases in which such procedure would be employed profitably. But when we regard the extreme inefficacy of our ordinary resources in combating this phase of ill health, Dr. Jackson's suggestions, which are supported by clinical evidence, should receive the most careful consideration.

Clinical papers follow upon "Cataleptic convulsions cured by trachelorrhaphy," and "Ovariectomy during pregnancy." The first of these papers narrates an extremely well-observed case by Dr. Sutton, of Pittsburg, and demonstrates the remarkable results that may sometimes be obtained by Emmet's operation for laceration of the cervix uteri. The second, by Dr. Wilson, of Baltimore, led to a curious diversity of comment in the discussion that followed—some of the members advocating non-interference in such cases, others ovariectomy as soon as pressure on the uterus begins, and

others abortion in place of ovariectomy,—this last suggestion having certainly the merit of originality. No conclusion was reached, although to most readers the sensible one seems obvious enough.

These papers are followed by one from Prof. Parvin, of Indianapolis, viz., "Secondary puerperal metrorrhagia," which, without invidious comparison, may safely be termed the most complete and scholarly essay in the volume. It is practical, accurate in its statements, and scientific in its methods, and could only have been written by a man who was at once a student and a keen clinical observer. To be appreciated it must be carefully read and studied.

Three fatal cases of rupture of the uterus with laparotomy are next narrated in detail by Dr. Howard, of Baltimore, who proposes ablation of the uterus by a modification of Porro's operation as a substitute for the methods now in vogue for treating this appalling casualty. The proposition is bold but not unreasonable.

"Occlusion of the gravid uterus," by Dr. Eve, of Georgia, and a long and extremely elaborate dissertation on "Posture in labor" among different nations, by Dr. Engelmann, of St. Louis, follow next in order. The latter of these is profusely illustrated, and contains a mass of more or less useful information, from which the author deduces the conclusion that in ordinary labor cases, the semi-recumbent position of the patient should be adopted.

After this comes a short but practical and intelligently written paper by Dr. Chadwick, of Boston, on the "Hot rectal douche," which he recommends for two distinct classes of cases—inflammatory conditions of the rectum and colon, and intrapelvic inflammations when accompanied by painful defecation and burning abdominal pain. Many clinical cases are given in corroboration of its beneficial effect.

This is succeeded by a dissertation on "Quinine in gynecic and obstetric practice," by Prof. Campbell, of Georgia, whose experience entitles his opinions to great weight. Like all who live and practise in highly malarious climates, he uses this drug more freely than in the North, and relies almost exclusively upon it.

Essays on "Manual dilatation of the os uteri," for the induction of premature labor, and upon the comparative value of "Laparotomy and laparo-hysterotomy for the removal of uterine fibroids," conclude the list of papers. The former, by Dr. Richardson, of Boston, is a plain and practical exposition of a method

well recognized by obstetricians as more rapid and effectual than the use of Barnes' dilators, etc., but in rash or unskillful hands fraught with the utmost danger. The second, by Prof. Palmer, of Cincinnati, is a careful and comprehensive study of hysterotomy, which will well repay perusal, and so complete that it must long remain of value to students or future writers on this subject.

The indices which conclude the volume are all exceedingly full and interesting; while that of "Gynecological and Obstetric Literature for 1879," is a monument of thoroughness and research. It reflects the utmost credit upon its authors, and would alone be worth much more than the cost of the entire volume to all students of gynecology. [C. C. L.]

Eczema and its Management. A Practical Treatise Based on the Study of Two Thousand Five Hundred Cases of the Disease. By L. DUNCAN BULKLEY, A.M., M.D., etc. New York: G. P. Putnam's Sons, 1881, pp. 344.

Eczema, comprising, as it does, fully one third part of all diseases coming under the observation of the dermatologist, and presenting itself under so many guises, has, naturally, claimed a large share of the attention of those who write about skin affections. Necessarily, all systematic treatises upon dermatology discuss eczema at length, and indeed, so important has the subject seemed, that several bulky volumes have been devoted to it alone. Thus, in recent years, we have had works on eczema by Erasmus Wilson, McCall Anderson, and the late Tilbury Fox, and now we have this latest contribution from Dr. Bulkley, which finds, in America at least, an almost unoccupied field; for whatever may have been the cause, the English works just mentioned have failed to receive the favor in this country that one might have expected.

In estimating the merits of the work before us, it will be properly considered as divided into two parts. The first part treats of general questions relating to eczema. There is a chapter devoted to general considerations, the definition of eczema and its nosology, and one to general statistics. Chapter iii discusses general symptomatology and pathological anatomy; chapter iv, forms of eczema; chapter v, its diagnosis and prognosis; chapter vi, the nature of eczema; chapter vii, its etiology; and chapter viii, the treatment of the affection. Of the remaining eight chapters, six are addressed to the management of infantile eczema, of eczema of the face and scalp, the hands and arms, the feet and

legs, of the arms and genital regions, of the trunk, and, finally, of the general surface. The book concludes with a chapter on the diet and hygiene of eczema, and one on therapeutics.

In the consideration of a subject so many-sided as eczema, an arrangement such as the above, although inconvenient, appears to be unavoidable, and calls for a larger number of pages than one might deem necessary or desirable ; and indeed, we think the author *has* been unmindful of the advantage of saying what one has to say as concisely and in as few words as possible.

In the first eight chapters the author has had abundant opportunity to express his views upon eczema generally, and more especially its pathological and etiological relations ; views which, without doubt, will not be subscribed to by many dermatologists. Eczema is stated to be "a non-contagious, inflammatory disease of the skin, of constitutional origin, acute or chronic in character, manifesting any or all the results of inflammation at once or in succession, and accompanied by burning and itching." Notwithstanding his belief in the constitutional origin of eczema, Dr. Bulkley proceeds to confirm the accuracy of those who claim eczema as a catarrhal disease of the skin, despite the fact that catarrhs of purely local origin are of daily occurrence.

In seeking to establish his theory of the constitutional origin of eczema, we find the author ruthlessly discarding from the class of eczematous affections, diseases whose local origin seems to be assured. In this way, some forms of eruption that in all physical characters are eczematous are pronounced to be *not so*. The eruptions upon the scalp and body produced by pediculi are claimed as always being "dermatitis." And yet it is difficult to believe that many of the eruptions in the lower occipital region accompanying pediculosis capillitii, are any thing but eczemas. Eruptions produced by heat and cold, animal poisons, various vegetable substances, such as croton oil, savin, poison ivy, and arnica, aniline dyes, etc., he says, are never eczematous, although they may become the starting-points of true eczema, which may then remain and relapse again and again without recognizable local cause. Surely one frequently sees eczema, pure and simple, arising from one or other of the above-mentioned causes ; and indeed, at another page the author, obliged to admit this, asserts that it can only occur in those who possess the eczematous predisposition. To this we will refer in another place. That eczema, thus produced, tends to recover with the removal of the exciting cause, is simply because the tendency of the parts, less the irrita-

tion, is toward health. An irritant from without may be readily and entirely removed, while one residing in the part itself must offer more resistance and produce a more prolonged disturbance of nutrition.

A very valuable statistical summary gives the author's experience upon many features of eczema. His observations of the comparative frequency of eczema correspond closely with the much more extensive figures of the Statistical Committee of the American Dermatological Association; eczema affording $34\frac{1}{4}$ per cent. of all skin diseases treated by him. Tables are given showing the ages of the patients, the duration of the complaint, the location of the eruption, the hereditary tendencies of the patients (respecting eczema). The author, for the most part, denies heredity in eczema. One table shows the frequency of the co-existence of eczema with other skin diseases. Of general diseases causing or accompanying eczema, the author found that malaria was almost without influence in this direction, while asthma affected eczematous patients so frequently, that he has come to consider "asthma, in many instances, simply a condition of the pulmonary mucous tract, similar to that found in the skin in eczema." It may be noted, as not a little strange, that while gout was detected with great frequency in eczematous patients, disease of the kidney, that so frequently results from the gouty state, was only rarely discovered; a statement that suggests the suspicion that the author's notion of the prevalence of gout is perhaps a little extravagant.

Chapter iii contains a description of the symptoms of eczema and of its pathological anatomy. This simply sums up the symptoms that may occur in the course of the disease, and no attempt is here made to give their clinical relations. Although Dr. Bulkley refuses to consider certain artificially produced eruptions as eczematous, he avails himself of the results of Neumann, from investigating a croton-oil eruption upon the ear of a rabbit, and, indeed, considers that the process does not differ from that of eczema. The pathological condition in chronic eczema, as studied by a number of histologists, is described, and attention is especially directed to the enormous distribution of nerve fibres to the skin, and to the probable influence of the nervous system in the pathogenesis of the disease.

No less than 125 Latin names have been collected by the author, that have been applied to the various phases of eczema. What wonder that so great obscurity has surrounded its study!

Order, however, is being gradually brought out of confusion. The author arranges the nomenclature in five classes of terms, to which is appended a sixth class of miscellaneous terms which will probably slowly pass out of use. These classes of terms relate : 1. To the stages of the eruption ; 2. To the predominant lesion ; 3. To the condition ; 4. To the cause ; 5. To the location. Thus, we speak of "eczema (1) chronicum, (2) papulosum, (3) fissum, (4) artificiale, (5) manuum." In the diagnosis of eczema, there are enumerated no less than 28 different affections liable to be mistaken for it. One of these is dermatitis (from heat, poison ivy, etc.), which, we are told, can sometimes only be distinguished from eczema by getting well quickly, and which may be the starting-point of a true eczema. Another affection, phthiriasis, may resemble pustular eczema very closely, "but the presence of pediculi and their nits is, however, sufficient for the diagnosis" (why not *call* it a pustular eczema from the irritation of lice?). The chapter on diagnosis is, however, well worked up, and we believe that about every point in diagnosis that can arise has been discussed. In the remarks on prognosis, eczema is pronounced to be a curable disease, but the author is wisely prudent in indicating circumstances interfering with recovery.

It is in his treatment of the question of the local or constitutional nature of eczema, that Dr. Bulkley will, undoubtedly, encounter much dissent. As we have seen, he unhesitatingly whisks overboard in preparing for action, various inflammations presenting the clinical features of eczema, as not eczematous. "If we accept one artificial eruption as eczema, we must accept all as such, from the large blisters following cantharides, heat, and cold, to the discrete pustular eruption produced by croton oil or tartar emetic, or the slightest erythematous blush caused by the mildest irritant." This is a remarkable statement. We usually recognize pathological processes by their general features and course, not from their exciting causes, and while we cannot recognize an eczema under all the conditions mentioned, we usually look upon an eruption presenting in every particular the features of eczema, as eczema ; and fail to see any justification for any other conclusion. But, moreover, while we do not consider all these forms of eruption as eczematous, we do believe that they, in the main, depend upon processes essentially similar, from the sudden inflammation rapidly followed by free exudation, caused by a cantharidal plaster, to the slow chronic eczema proceeding from an irritation within the part itself. The difference lies in the intensity and transient char-

acter of the irritation upon the one side, and upon its mildness and persistence upon the other. In concluding that eczema cannot be a local disease at one time and a constitutional disease at another, the author surprises us, after the observations we have just quoted, in saying "that the eruptions resembling eczema, artificially produced, are either ordinary dermatitis with a strong tendency to spontaneous recovery, or are true eczema in eczematous subjects, in whom the exciting cause, instead of recurring in the ordinary way, has been artificially supplied, just as a gouty person might, by measures voluntarily applied, induce a true gouty inflammation of a joint."

The author proceeds to discuss the nature of eczema under seven headings. He first compares eczema with the acknowledged constitutional disorders of the skin, leprosy, contagious fevers, syphilis, etc. Here he finds resemblances in the symmetry of the lesions, etc. The conditions of the existence of these affections are so different, however, that comparison can shed no light on the pathology of either. Eczema does not depend upon a particular principle, as we have every reason to believe the first-named affections do. Even if "the peripheral mode of spreading of eczema resembles much that of erysipelas," it can in this connection have no significance, since the processes are totally unlike. "Eczema is also not unfrequently attended with fever in its more acute and general forms." So is a cut finger. In contrasting certain local diseases of the skin with eczema, the former are claimed to be absolutely unsymmetrical. We would mention, as proving the contrary, two or three that just occur to us. These are acne, seborrhœa, hyperidrosis, some erythematous eruptions, even common warts, which are often decidedly symmetrical, or at least sufficiently so to equal in this respect many eczematous eruptions. From the study of microscopical anatomy, which is discussed at considerable length, absolutely no evidence favorable to the theory of the constitutional nature of eczema is to be found. Indeed, the author limits his inquiries to conjecturing whether the impulse to inflammation resides in the cells primarily and is only secondarily extended to the blood-vessels (cellular pathology), or whether the vascular disturbance is primary, and to what extent these processes depend upon nerve influence; questions of immense importance, it is true, but which have no bearing upon the point that the author seeks to establish, since in neither process is there involved, necessarily, a general blood alteration.

The next point considered is "the constitutional relations of

eczema as exhibited in its clinical history." It occurs at any age ; as frequently in the female as in the male. It relapses. We are told that eczema patients seldom pass large amounts of urine, the tendency being to scanty secretion, almost always unnaturally acid, with a specific gravity averaging above normal. It is unfortunate that Dr. Bulkley has not supported this assertion with a statistical table, such as he has so carefully prepared in relation to other features of eczema.

In discussing the theory of local pathology, Bulkley quotes from Hebra's latest edition, statements that go to show that this great dermatologist was willing to admit that there are certain conditions of the human organism that act as predisposing causes in the production of eczema. We insist, however, that the author utterly fails to grasp the true meaning of Hebra, and that there is a vast difference between disease of a part resulting from lowered constitutional conditions and disease of a part resulting from a definite constitutional disease. It would be absurd to deny that local diseases are largely under the influence of general conditions. Wherever we turn we find unmistakable evidence of this influence. Not to speak of those skin affections where the nervous system seems to supply the bond between them and diseases of various organs, every one must have seen skin affections improve with a general improvement of health, or relapse as the general health became impaired, or must have observed signs of cutaneous debility appear for the first time in those whose previous good health has begun to fail. But there is every reason to believe that in these affections simple imperfect nutrition and assimilation are sufficient to evoke the symptoms in a skin of deficient resisting power, just as they may evoke symptoms in other organs—the *loci minoris resistentiæ*—of the individual, without calling in, to become responsible for them, any specific blood alteration. That the skin of one person is more liable to suffer than that of another, is due to certain histological and nutritive imperfections in the skin of the particular individual, and we find similar deviations from health wherever we look for them. Many persons are more liable than others to disorders of the pulmonary mucous membrane, of the heart, of the kidneys, of the different portions of the alimentary canal, and this liability often remains throughout life ; and yet no one dreams of considering the morbid conditions thus arising, as dependent upon any specific morbid condition of the general system. (We are, of course, speaking of liabilities that have not been acquired through certain disease processes, as rheumatism,

for example). This conception of the influence of the constitution in the production of eczema, differs widely from that of Bulkley, who believes, "that as arthritic, pulmonary, or cerebral symptoms appear to be the culmination of blood processes which we know as gout and rheumatism, so eczema is directly dependent upon a somewhat similar, although as yet but little defined blood change," etc. At the end of this paragraph we find the inconsistent statement, "that the constitutional state may pass away spontaneously or under dietary, hygienic, and medicinal measures, while the *products* of the disease, the infiltration and consequent itching, remain." This, in the face of an earlier observation, that so long as the infiltration remains the eczema is not cured, but will pretty certainly return upon the suspension of treatment.

It is to be remarked that the author in directing constitutional treatment does not address his efforts to any peculiar eczematous systemic condition, but cures one case by giving diuretics and cathartics for renal disturbances, nitric acid for "biliousness," remedies addressed to the nervous system, where that system is at fault, cod-liver oil to the scrofulous, iron to the anæmic. In a word, the disease is vanquished by restoring healthy nutrition to whatever portion of the organism has been at fault.

In considering the etiology of eczema, the author asserts that careful and repeated study and observation of patients with eczema will always show that they are not in perfect general health. The constitutional state "is one of debility, or lowered vitality of the whole system, or of one or more portions." We read that the pulse is rarely healthy, but may be variously abnormal; but as it betrays no special characters, it can hardly be expected to throw much light on eczematous processes. Eczematous patients, we are told, may be divided into three tolerably distinct classes: those who are gouty, those who are scrofulous, and those who are neurotic. The recognition of these states is not always easy, but is important as bearing seriously upon questions of therapeutics. The gouty state as a cause of eczema receives extended notice. Then follows a long essay upon conditions of imperfect digestion, etc., which, though doubtless important and valuable, seems rather out of place in the present work. Undoubtedly many scrofulous children are eczematous, but then they likewise develop all sorts of other affections due to their scrofulous taint, displaying faulty nutrition in all parts. So, also, are many eczematous persons victims of nervous debility in various forms, as the author indicates; and it is of interest to observe that the symmetry so often ob-

served in eczematous eruptions he regards as evidence of the nervous relations of the disease, a supposition that is probably correct, and which deprives his theory of a blood disturbance of one of its props. As exciting internal causes of eczema are mentioned : indigestion, menstrual difficulties, lactation (producing a debility that results in the disease), dentition, varicose veins, and certain chronic diseases. It is difficult to believe that the author seriously considers the eczema so frequently developed in the legs of persons suffering with varicose veins of the lower extremity, as depending upon a special constitutional vice. Similarly, we suppose, he would attribute a like origin to varicose ulcers. Too much stress, he tells us, has been laid upon the importance of local causes in producing eczema. He considers their influence to be very slight, though he gives the usual list, with the reservation, that eczema results from such causes only in persons predisposed to the disease ; a manifestly true remark, which really means that the skin of some persons is more readily excited to eczematous inflammation than that of others, and implies, not the presence of a definite morbid condition of the blood, but a diminished power of resistance of a part.

Indeed, in considering the author's views concerning the pathology of eczema, we confess that we do not precisely grasp his meaning. He repudiates the older humoralistic doctrines, in claiming a constitutional blood origin to the disease. "This constitutional state is not represented by any one single definite condition ; there is no dyscrasia proper to eczema, as far as can be determined at present, although, for convenience, the term eczematous diathesis may perhaps best represent the totality of systemic conditions which are found in subjects of eczema." In support of this theory we are offered evidence that eczema occurs under a variety of conditions, in the scrofulous, in the gouty, in the neurotic, under circumstances of pronounced debility, or where the signs of imperfect health must be searched for with microscopic eye, but not a jot of proof that this hypothetical "diathesis," this specific blood condition, has a single sign of its own, except the cutaneous manifestations, which are far more reasonably accounted for by conditions of imperfect nutrition of the parts themselves, whether through faulty innervation, or whatever influence presides over their life.

It must be conceded, however, that his views of the pathology of the disease do not turn the author from sound principles of treatment. For the eczematous condition he has no specific. For

the acute stage we find recommended, mild laxatives and diuretics, and alkaline mixtures,—such treatment, in fact, as is employed in any mild inflammatory disorder. In chronic eczema the gouty, strumous, or neurotic state must be corrected. Much that he says of the constitutional management of eczema the author admits “pertains as well to other diseases and to persons exhibiting no eczema at all.” The general health must be improved, the frequently existing constipation must be cured, the liver must be restored to healthy function. A valuable observation is the condemnation of the free use of enemata, as not meeting requirements, and the preference expressed for purgatives. Rational measures for the correction of digestive and urinary derangements are counselled. Arsenic is held in high estimation, though not for every case, and certainly not to the extent that is popularly believed in. It is strange that, despite the influence of nearly every modern dermatologist, arsenic continues to be the remedy persistently employed for every skin disease. To relieve itching gelsemium is especially recommended. The tincture should be used in ten drop doses, repeated every half hour until relief follows or until unpleasant physiological symptoms arise. If further experience confirm this action of gelsemium, it will prove a most valuable acquisition to our therapeutics. The directions for local treatment show the results of a large and carefully considered experience, and cannot fail to be of the greatest value to the profession.

It is with a sense of relief that we pass from general questions concerning eczema to the consideration of special forms of the disease and their management. Here we reap the harvest of the author's clinical experience, freed, to a great extent, from theoretical considerations. The chapter on infantile eczema has been very carefully prepared. The baseless fear of “driving in” an infantile eczema is exposed. Special attention is directed to the frequently associated formation of abscess, especially on the scalp and in debilitated children during the summer. These are manifestly due, though the author does not mention it, to the congestion of the sweat-glands, consequent upon their increased activity in warm weather, and are most often seen in the children of the poor in the alleys and crowded dwellings of large cities. While admitting that most eczematous children appear to be otherwise healthy, the author claims that a very rigid scrutiny will reveal some departures from a perfectly normal condition. And yet the deviation is, in many cases, very trivial. The highly detrimental

effect of the free, local use of water in these cases is insisted upon. Indeed, the subject is treated generally in a masterly manner.

Eczema of the face and scalp is described at considerable length. Of the author's skilful handling of this portion of his subject we have only praise. The directions for treatment are minute and comprehensive. Of all curable cutaneous affections, eczema of the hands is probably the most unamenable to treatment, and when cured, probably the most prone to relapse. This is readily explained by the liability of these parts to all sorts of irritation. The author's experience in the management of this form of eczema can but be valuable, but we fear that the hopes of the practitioner will be often disappointed, as heretofore, and unless a total change can be effected in the manner of life of the individual, his patient will often carry his eczema indefinitely. We think, however, that the therapeutic measures offered in this work promise as much relief as may be reasonably demanded. Eczema of the feet and legs deserves especial attention from the influence of position in its production. This is especially when the force of gravity, opposing the return of blood to the heart, is accompanied by dilatation of the veins. Eczema is a most common result of these passive congestions, especially when reduced general health further enfeebles the circulation. No plan of treatment that disregards this circulatory impediment will secure more than passing relief. The tissues must be supported, and to effect this, nothing equals the use of Martin's solid rubber bandage, first employed for this purpose by the author. The details for its application are given. It often affords incalculable relief. A method for the relief of the persistent and intolerable itching of eczema of the anus and genital regions, suggested by the author, is well worth remembering. The part should be enveloped in a handkerchief soaked in water as hot as can possibly be endured. This should be repeated at short intervals for several minutes, when, after careful drying, appropriate ointments should be applied. Cosmoline and vaseline, from which so much was expected as a basis for ointments, the author does not employ, on account of their not having body enough to remain as a thick coating on the cloth. They rapidly soak in, and leave the parts dry and exposed.

The final forms of eczema treated of are eczema of the trunk and general eczema. The eczema of the nipples and areola, described by Sir James Paget as often preceding cancer of the breast, receives due attention, as do also the various forms of general ec-

zema. Diet and hygiene of eczema are discussed in twenty-eight pages. This we consider inexcusable. We have already had occasion to object to the long disquisition upon gout and indigestion in an earlier part of the work. Not that we object to what the author says about these subjects. We willingly concede the accuracy of his remarks. But they might, with equal propriety, be applied to the discussion of any question of diet and hygiene, and bear no more upon eczema than upon a dozen other affections. In a work devoted to a special subject, one does not expect to have the number of pages needlessly multiplied by essays upon related, though not essential topics. The reader prefers to acquire his information upon these matters in the places where they properly belong, and to which the specialist would always do well to refer him, after having indicated their bearings upon his subject. The formulary contained in the concluding chapter will be found very valuable.

[I. E. A.]

Lectures on the Pathological Anatomy of the Nervous System—Diseases of the Spinal Cord. BY J. M. CHARCOT. Translated by CORNELIUS G. COMEGYS, M.D. Cincinnati: Peter G. Thomson, 1881.

The subject-matter of the book of 160 pp. before us appeared in the *Progrès Médical* during 1879–80, and consists of lectures delivered at the École de Médecine by Charcot, reported by Dr. E. Brissaud.

To criticise a treatise on the spinal cord by Charcot would be an impertinence, and with regard to the work as a production of the author, it need only be said that it should be in the hands of every medical man not reading French. As a *résumé* of the known physiology, regional anatomy, and localization of systematic and other lesions of the spinal cord, particularly ascending and descending degenerations, it is, according to our present knowledge, perfect.

As a reproduction by translator and publisher it is open to criticism. The work of a translator, who puts at our command a valuable foreign production, is at best poorly paid and often thankless, and hence we should be led to temper our criticism with a grateful acknowledgment of our indebtedness.

The translation is good, with the exception of some very frequently recurring alliterations, as "inutile," "primitively," and "conservation," p. 64; "interested," p. 80; "biceps crural," p. 105; and such evident mistakes as "sensitive" for sensory, p. 49;

"nervous cell," p. 55; "locomotive ataxia," p. 128; and "contraction" for contracture, p. 131.

His use of "rotulian" for patellar is, of course, only a matter of taste. His word "reflexion" (our commonly accepted reflex) would look better, if he insists on a substantive, as reflection. The introduction of "lesed" as a past participle is, it seems, a rather disagreeable coinage and hardly a substitute for injured.

A word now to the publisher. In the last few years several translations, particularly two by Fowler, Charcot's *Localization in Diseases of the Brain*, and Benedict on *Brains of Criminals*, both published by William Wood & Co., New York, together with the present publication by Thomson, of Cincinnati, have thrust into the sensitive view of an American public wood-cuts vying in ugliness with the meat-axe productions of the daily press.

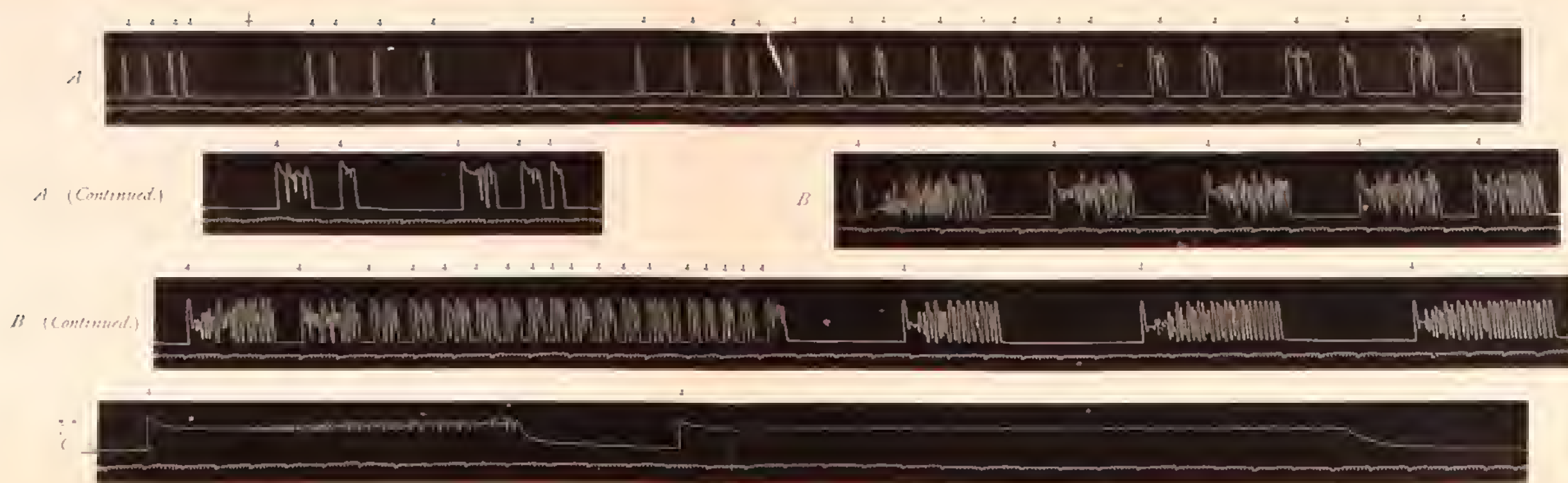
The cuts in the translations of Benedict are sickening parodies on the photo-lithographs of the German edition. It may be said of the cuts in the two translations of Charcot that they are fair reproductions of the originals, but that is not enough.

The style and finish of wood-cuts habitually seen in the *Progress Medical* are not acceptable in a country which leads the world in wood-cutting. But further, when the primarily coarse stippling presents the appearance, in the reproduction, of having been made with a rock-drill (see Wood's Charcot, fig. 37, p. 123, and Thomson's Charcot, fig. 30, p. 79), and attempts at tint work in the originals are executed in the reproductions by an oyster-rake see (Wood's Charcot, fig. 21, p. 72, and Thomson's Charcot, fig. 15, p. 47), then comes the time for remonstrance against the parsimonious ways of certain publishing houses.

A wood-cut can be a perfect reproduction, as far as purposes of demonstration go, and at the same time be artistically far superior to the original, at a very little extra cost to the publisher.

[R. W. A.]

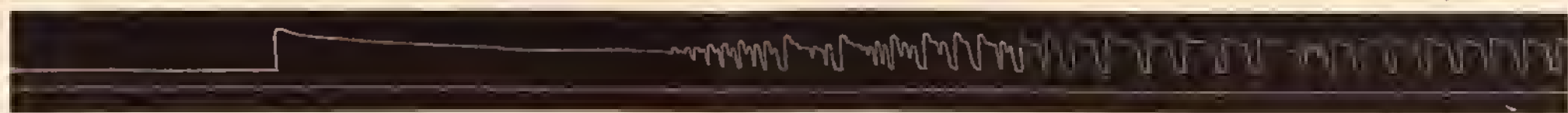
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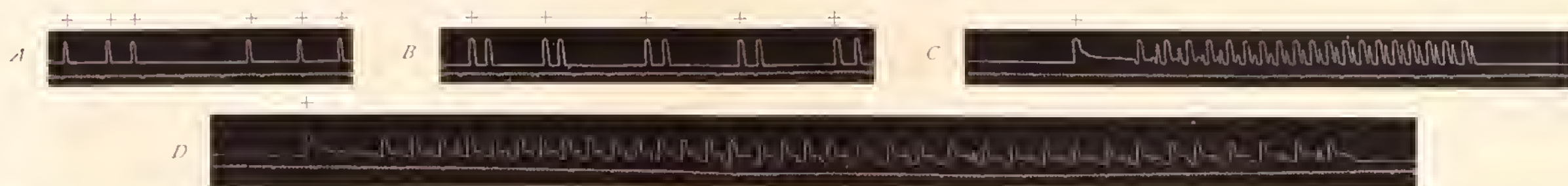
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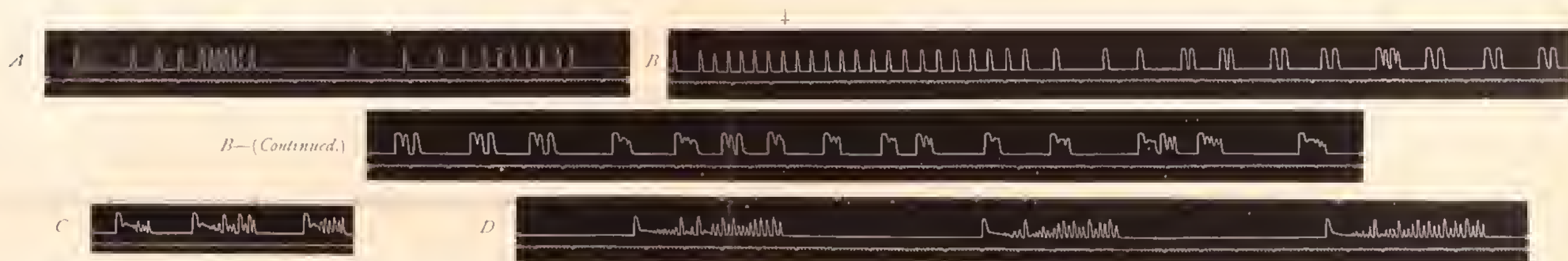
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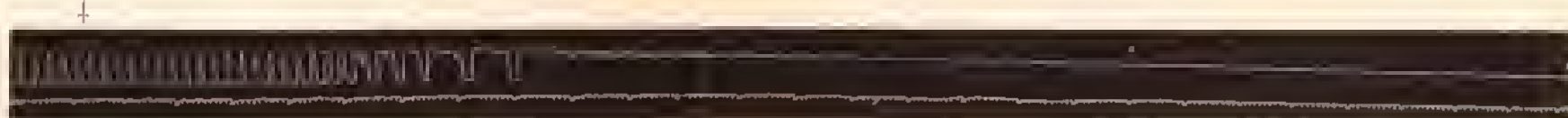
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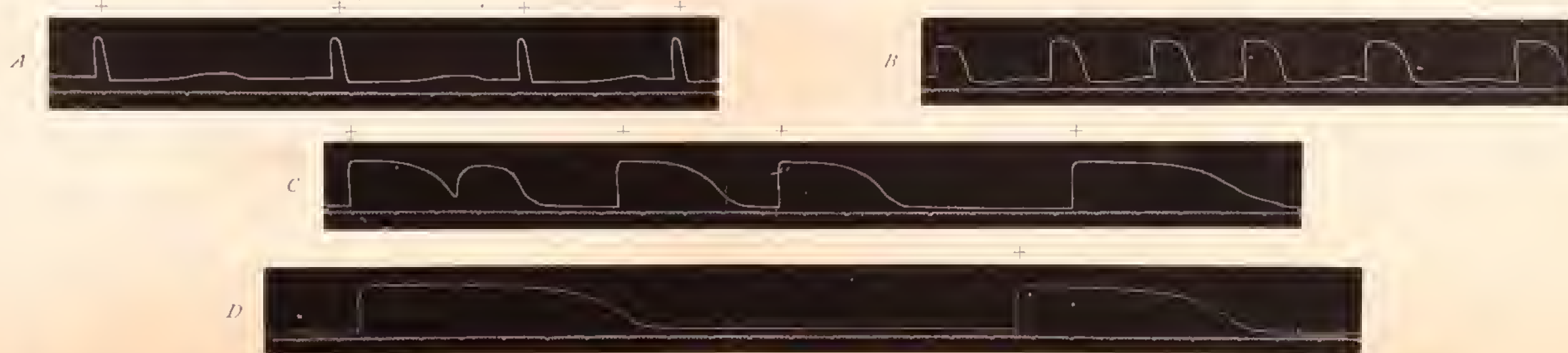
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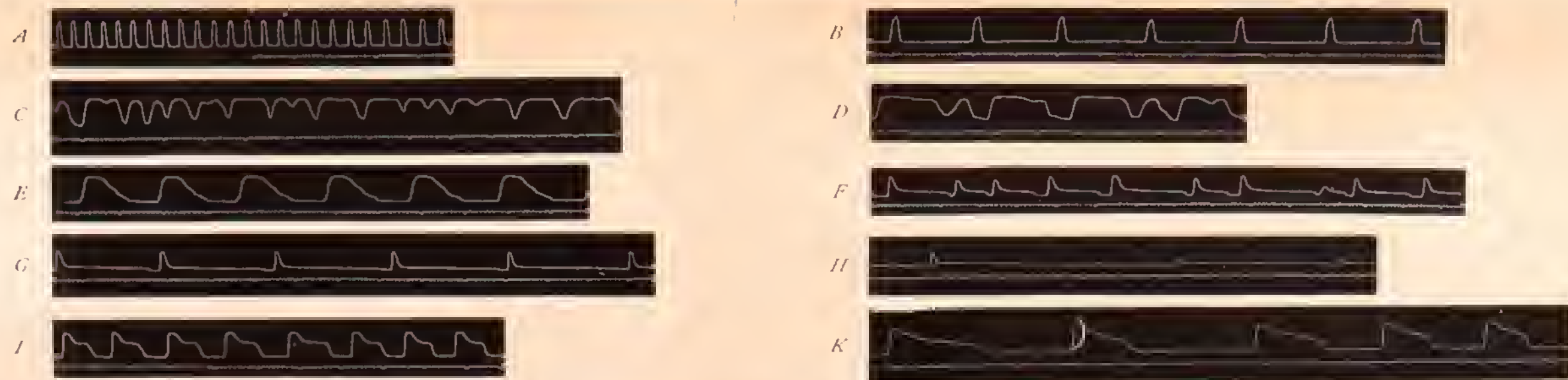
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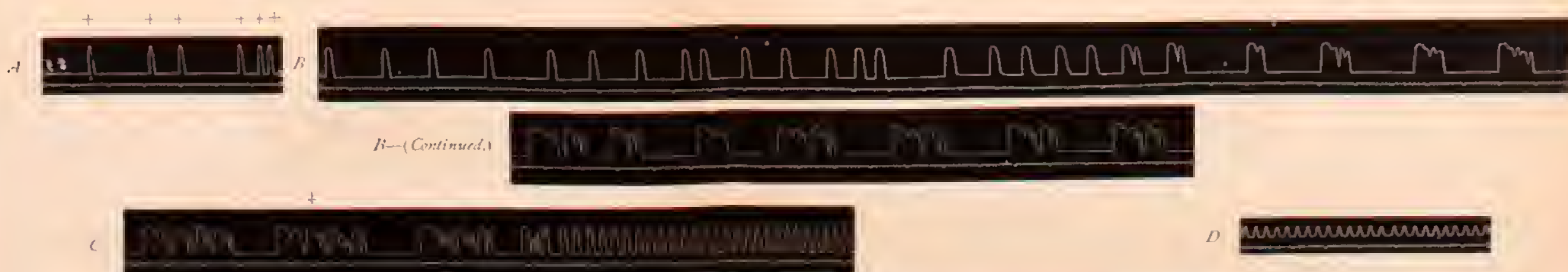
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TRACE IX.



ARCHIVES OF MEDICINE.

Original Articles.

NEUROSES OF SENSATION OF THE PHARYNX AND LARYNX, OR SENSORY NEUROSES OF THE THROAT.*

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SECTION III.—CLASSIFICATION.¹

THE various sensory neuroses of the throat have not hitherto been strictly differentiated. This has been mainly because the three different kinds of sensibility of the throat have not been clearly appreciated. Although they are frequently affected together, each kind may be diminished, increased, or perverted separately; and an exaltation of one kind may be combined with a diminution or perversion of another. The correct position, in the classification, of neuralgia seems to have presented difficulties to observers, and systematic authors on the subject, such as Ziemssen, Mackenzie, Jurasz, etc., have described it as a fourth disorder of sensibility, separate and distinct from anæsthesia, hyperæsthesia, and paræsthesia.

Comprising disordered sensibility under the name dysæsthesia, my classification, based on clinical observations, not formed only "from theoretical considerations," is as follows:

* By special appointment prepared for, and partially read before, the International Congress in London.

¹ Continued from these ARCHIVES, February number, p. 57.

DISORDERED SENSIBILITY : DYSESTHESIA.	A. In reference to quantity or degree of sensation.	I. Diminished sensibility : Hypæsthesia.	1. Anæsthesia. 2. Analgesia (Analgia, Anodynia). 3. Anæsthesia dolorosa. 4. Tactile hypæsthesia and hypalgia. 5. Reflex hypæsthesia.
		II. Increased sensibility : Hyperæsthesia.	1. Tactile hyperæsthesia. 2. Hyperalgia (algæsthesia, hyperalgæsthesia). 3. Reflex hyperæsthesia.
			{ Peripheral. } { In nerve trunk or } { Paroxysmal or } { true neuralgia. } { Cough, choking, spasm, retching, gagging, etc. } { Muscular effects. } { Circulatory effects. } { Secretory effects. }

The perversions of sensibility are so various that it is impossible to classify them or to enumerate them all. There are, however, perversions of tactile, of dolorous, and of reflex sensibility. Among the first mentioned, the most frequent are the sensation of heat or burning (the opposite—a sense of coldness—is very rare, but I have also seen that) and the sensation of the touch of a foreign body, attention being given mainly to its shape and its weight or pressure. Patients describe all possible and impossible shapes and materials in endeavoring to give an account of the perverted sensations. I have sometimes thought most of them might be arranged under the three heads: 1. Sphæræsthesia. 2. Zonæsthesia or strangalæsthesia, and 3. Akanthæsthesia; comprising under the first, the sensation of a bulky body, generally ball-shaped; under the second, a cincture or band feeling, a sense of constriction; and under the third, the sensation of a sharp-pointed body, piercing or cutting. I shall give a few further particulars when speaking of symptoms.

III. Perverted sensibility :
Paræsthesia.

SECTION IV.—OCCURRENCE.

a.—FREQUENCY.

“Anæsthesia, hyperæsthesia, paræsthesia, and neuralgia of the larynx undoubtedly may exist to a more or less well-marked extent, but whether they occur as separate and distinct affections, and are entitled to consideration as such, is certainly an open question. * * * That paralysis, due to any morbid condition of the superior laryngeal nerve, such as neuritis, pressure of tumors, diphtheria, etc., will produce loss of sensation in the parts to which the nerve is distributed is undoubtedly true; but so long as this disease is described as one which might possibly occur rather than one which has been clinically observed, it would seem that its introduction into a treatise on these affections would be something of a refinement in classification.” Such is the language of the most recent “Manual of Diseases of the Throat and Nose.”¹ The fact is that sensory neuroses of the throat, though relatively infrequent, are not of very rare occurrence. Excluding the cases not coming fully within my definition and the explanation given, I have had under observation *at least* 58, which I have classified as follows: *Hypæsthesia*, 10, viz., complete one-sided anæsthesia, with hypæsthesia of the other side, 1; analgesia, 2; anæsthesia dolorosa, 1; tactile and reflex hypæsthesia, 3; reflex, without tactile or dolorous, hypæsthesia, 1; hypæsthesia with localized hypersensitiveness to temperature, 1; tactile and reflex hypæsthesia with some hyperalgesia and paræsthesia, 1. *Hyperæsthesia*, 27, viz., tactile and reflex hyperæsthesia, 10; tactile without reflex hyperæsthesia, 1;

¹ By Francke Huntington Bosworth, A.M., M.D., New York, William Wood & Co., 1881, p. 328.

In the preface the author says the book embodies the results of an experience of nearly ten years and of over eight thousand cases. According to my personal experience, in such a number there ought to have occurred a score of unquestionable instances of sensory neurosis. Perhaps I must not omit to state that Dr. Bosworth's cases were mainly those of a hospital out-door clinic.

tactile and reflex hyperæsthesia, with hyperalgesia from only external impressions, 2; tactile and reflex hyperæsthesia, with algesia both spontaneous and from external impressions, 2; neuralgia, 9; reflex, with very little or no tactile and dolorous hyperæsthesia, 3. *Paræsthesia*, 21.

As to sex and age, there were 26 male and 32 female patients, of whom the three youngest were a girl of seven and a half and two boys of eight and nine years respectively, and the two oldest a woman of 58 and a man of 65 years. Altogether there were

Under 10 years, 3 cases, viz., 2 male and 1 female.									
Between 10 and 20	"	7	"	2	"	"	5	"	
" 20	" 30	" 13	"	5	"	"	8	"	
" 30	" 40	" 19	"	8	"	"	11	"	
" 40	" 50	" 11	"	5	"	"	6	"	
" 50	" 60	" 3	"	2	"	"	1	"	
Over 60		" 2	"	2	"				

The greatest number of both sexes were between 30 and 40 years old. The rise and fall as to age in the number of male patients is remarkably uniform. The number of females exceeds that of males in every decade except in those under 10 and over 50; the total excess is not more than 23 per cent., which is much less than is usually supposed to be the case in such neuroses.

b.—DISEASES OR PATHOLOGICAL CONDITIONS IN CONNECTION WITH WHICH SENSORY NEUROSES OF THE THROAT HAVE BEEN OBSERVED.

1. *Paralysis*.—Although the term paralysis embraces impairment of sensation as well as of motion, yet when used without qualification, only deranged muscular activity, either neuropathic or myopathic, is usually meant. Throat dysæsthesiæ, *i. e.*, anæsthesia, hyperæsthesia, and, to some extent, paræsthesia, occur in many cases of paralysis; they have been observed in cases of laryngeal paralysis from

disease or injury of the pneumogastric nerve. I have met with them very early in a case of progressive paralytic dementia, *i. e.*, paralysis of the insane, in the course or later stages of which disease they are known to every alienist. Krishaber has reported them in early, and Ziemssen in advanced, progressive bulbar paralysis, *i. e.*, Duchenne's progressive paralysis of the tongue, velum, palate, and lips, of which I have seen two cases, one of which I have watched from beginning to end.

I have had an unmistakable case of paralysis of the superior laryngeal nerve, which I have not included in the number of cases mentioned in this paper, because the notes of it—though a hospital case—are unfortunately mislaid. I have had an opportunity to observe a case of paralysis of central origin involving one side of the body and deglutition, breathing, and phonation as well as sensation of the throat.

In a case in which another surgeon had performed œsophagotomy for the successful removal of a set of artificial teeth, I found hypæsthesia and paræsthesia combined with paralysis of the inferior laryngeal nerve. Of the paralysis of hysteria, diphtheria, syphilis, etc., occurring in combination with these sensory neuroses, I need not here speak as I am about to take up these diseases separately.

Dr. A. H. Smith has made the ingenious suggestion that in some cases of paræsthesia "the phenomena might be explained by assuming that there was a slight paresis of some of the muscles of the throat, leaving others without sufficient antagonism."

2. *Hysteria, hypochondriasis, and neurasthenia.*—These morbid conditions, more than any other, are regarded as being connected with the dysæsthesiæ under consideration. Certainly nervous exhaustion, from excessive intellectual exertion or emotional or sexual excesses, masturbation,

etc.; the curious nervous condition called hypochondriasis or sometimes pathophobia or psychical hyperæsthesia; and the still more mysterious hydra-headed disorder called hysteria, give rise, directly or indirectly, to the most strange sensory neuroses; nevertheless a large proportion of the sufferers from throat dysæsthesiæ are not only not at all hysterical but also not in any way "nervous." According to Chairou, anæsthesia of the epiglottis and of the pharynx is so constantly present in hysteria that he regarded it as a pathognomonic sign. Sawyer also insisted on the frequency of its occurrence. On the other hand, Mackenzie said he had observed slightly diminished sensibility of the pharynx, but never that the mucous membrane of the larynx was at all obtuse to direct impressions; whilst Semon, Mackenzie's German editor, acknowledges that he has seen it in several cases, though not in every case.

The truth is, anæsthesia is by no means the most frequent disordered throat sensation which is met with in hysteria. Thaon found it in about one sixth of the cases. Hyperæsthesia, as well as paræsthesia, are far more frequent. To the latter belongs the well known *globus hystericus*.

3. *Chlorosis and anæmia*.—Closely related to the cases of nervous exhaustion in which sensory neuroses of the throat occur, are those of chlorosis and anæmia; the former especially connected with menstrual troubles; the latter, in both sexes, after great loss of blood, after severe illness, etc. The forms usually met with are those of hyperæsthesia or else neuralgia, but occasionally also tactile and reflex hyperæsthesia and paræsthesia.

4. *Diphtheria, syphilis, and malaria*.—The fact that more or less complete anæsthesia of the throat may follow in the wake of diphtheria has long been known. Careful and accurate observations as to this condition, were, however, first made by Ziemssen, afterward by Schnitzler.

Paræsthesia may accompany hypæsthesia. As to syphilis, Ott has published a detailed account of an interesting case.

I have had under my care a case of localized painful hypæsthesia—anæsthesia dolorosa—and another of neuralgia, in both of which no other cause could be recognized than malaria.

5. *Drug poisoning*.—Aside from general anæsthetics and narcotics, there are drugs which affect the sensibility of the throat. The paræsthesia produced by some, as for instance, the sensation of dryness by belladonna, of tingling by aconite, of constriction by nux vomica, etc., are well known. Bromides produce hypæsthesia of the throat, and in a case of saturnine aphonia which I have treated, there was said to be anæsthesia of the throat, which, unfortunately, however, I did not sufficiently carefully examine to record.

6. *Genito-urinary, pulmonary, and other diseases*.—Disordered sensations of the throat sometimes depend upon disease in other portions of the body. Cases in which this occurs are of two kinds: those in which a special nervous connection between the organ affected and the throat is known to exist, and those in which such a connection cannot be traced. A common instance of the first kind is a throat dysæsthesia from ear trouble. A still more frequently observed instance of the second is a throat dysæsthesia from genito-urinary, particularly uterine, disease. The first is generally supposed to be easily explicable as a "reflex," starting from the auricular branches and ending with the sensitive laryngeal branches of the pneumogastric nerve; while the second is admitted to be a recondite sympathy. But the first is not always easy of explanation: for, a reflex action such as this would be opposed to received physiological principles. As Hart has shown,¹ for a reflex action

¹ *Practitioner*, London, July, 1878, p. 342.

afferent and efferent fibres are necessary ; the former are of necessity sensory ; the latter may be motor, vaso-motor, vaso-inhibitory, cardio-inhibitory, or secretory. They are never sensory, for the simple reason that a sensory nerve is always afferent ; and Hart suggests, in place of the reflex theory, the hypothesis of extension of irritation from one nerve centre to an adjacent one. Such phenomena may also be due, as Woakes suggests,¹ to "vascular distension of the sensitive tissues in the region where the pain is appreciated, brought about by implication of vaso-motor nerves."

I agree with Woakes that "though some links in the chain may here and there be missing,—and some lines of impulse may be wrongly traced,—yet when a larger knowledge of the anatomy and physiology of the vaso-motor system is attained, allowing these errors to be rectified, the theory here broached will hold its ground and prove of much wider applicability."

In other cases, where there is exalted impressionability of the nervous apparatus of the throat, the explanation of a dysæsthesia may be that suggested by Arndt² for so-called "co-sensations" or paradoxical sensations. He says: It may happen that a certain excitant which acts upon a particular nerve produces not alone a sensation in the part of the sensorium to which it passes, but that the impressions of this part, on account of the exalted sensibility of another part more or less connected with it, are taken up by this other part. In such cases there are produced, aside from the normal sensation, others, *i.e.*, abnormal ones. Thus, for instance, a slight pressure of the boot produces not alone sensation of pressure in the foot, but headache, etc., etc.

¹ "On Deafness, Giddiness, and Noises in the Head," by Edward Woakes, M.D., London, 1880, 2d ed., p. 79.

² Eulenburg's "Real Encyclopædie der Gesamten Heilkunde," vol iv, p. 532.

Disordered sensations of the throat also occur in pulmonary affections, especially during the course of pulmonary phthisis, and even very early; being more distinct on or limited to the side of the affected lung, and under circumstances when organic laryngeal lesion must be excluded. Jurasz has recorded a case in which they formed a complication of croupous pneumonia.

I have stated that I exclude from present consideration the diminished sensibility during epileptic paroxysms, but sometimes this hypæsthesia persists, and, as in Spaack's case, is observed in the interval.

Rheumatism and gout have been accused of leading to sensory throat neuroses.

When foreign bodies have been lodged in the throat, though removed and though no traumatic effects have been produced, they often leave the sensation of their presence for a long time, and sometimes the mere belief that a foreign body has entered the throat makes the person feel similar symptoms.

Occasionally catarrhal and other organic throat affections leave disordered sensations behind;—and here it must be emphasized that whenever such local disease is found to co-exist with them, they are out of all proportion, sometimes even in no relation therewith, persist after the local lesion is cured, or sometimes are cured while the lesion remains.

Throat dysæsthesiæ dependent on various diseases of the body are discussed in my essay, published many years ago, on "The connection of throat and other diseases."¹

SECTION V.—CAUSES.

In the list of the morbid affections leading to sensory neuroses of the throat I have given a number of causes which, to avoid repetition, I shall not mention now. Pre-

¹ New York, 1870, a reprint from *Medical Gazette*, Jan. 22, 1870.

viously I had stated that, excluding the structural throat lesions which themselves involve the peripheral sensitive nerves and the brain lesions of insanity, there must be affected, in the neuroses under consideration, the respective nerve tracts or their nuclei; that such affection may come from traumatism, from pressure by tumor, foreign body, etc., from circulatory perturbation, or from other disease or injury, I need not dwell upon. Certainly the great ordinary *predisposing* cause of these neuroses is that general nervousness of constitution which is really "the child of civilization and mental culture, of refinement in clothing, food, and dwelling-place, of want of proper physical and muscular exercise," and which has been perpetuated by hereditary influences. Patients may belong to this class of nervous individuals without being at all hysterical or hypochondriacal, or what is commonly called "nervous"; yet they are more than ordinarily impressionable, are of so-called "neuropathic disposition." Of the *exciting* causes, the most frequently assigned by patients themselves is "taking cold"; and though that phrase is sometimes only ignorance-cloaking and meaningless, nevertheless exposure to atmospheric change, draft, the extremes of heat and cold, especially sudden change from one to the other, combined with wet,—is often, in persons predisposed, the only etiological starting-point which the most careful scrutiny can detect. Sometimes this is true of inhaling smoke or dust, or of taking at particular times alcoholic stimulants, or wine, or even coffee, in cases in which not the slightest local effect on the mucous membrane can be detected. Intense intellectual labor or emotional or other excesses, which usually can be regarded as predisposing causes, also occasionally act as exciting causes. I have had a case in which great sexual excitation in a perfectly healthy unmarried female was followed immediately by intense pain in the

throat, which required months of treatment before it could be relieved. Since she has been married, every sexual intercourse causes a temporary return of the throat pain.

When speaking of the fact that foreign bodies, after removal, sometimes leave for a long time disordered sensations behind, I referred to the curious circumstance that sometimes the belief of the presence of a foreign body causes the same subjective symptoms. It almost seems that in nearly every case psychical, either as predisposing or exciting, co-act with other causes in producing a throat dysæsthesia. Strong emotions always increase existing dysæsthesiæ, and I have had abundant proof that they alone, under many circumstances, are sufficient for their production. In families in which one or more members have suffered from severe, especially chronic, throat disease, either constitutional or local, I have several times found a sensory neurosis of the throat affect persons who were and remained absolutely free from structural lesion. In such cases, fear alone seems to be the efficient cause. Just as motor disturbances are recognized, that are dependent on an idea, on emotional attention directed strongly upon a particular part of the body, so such sensory derangements occur, entirely independently of hysteria, hypochondriasis, simulation, or even imagination, so far as this latter word carries with it the idea of unreality. Continued examination of the throat sometimes makes it temporarily oversensitive; and in some cases hyperæsthesia is observed without any cause, as an idiosyncrasy.

SECTION VI.—SYMPTOMS AND DIAGNOSIS.

The symptoms are mainly subjective, but in both hypæsthesia and hyperæsthesia, there are also objective symptoms, especially when the reflex sensibility is affected.

1. *Hypæsthesia*.—Diminution of sensibility may vary in

different cases from a slight bluntness of feeling to a complete absence ; and it is only in the latter case, *i. e.*, in that of loss of sensation, that the term anæsthesia should be used. The diminution may be more or less circumscribed or extensive, unilateral or bilateral, or, if on both sides, greater on one side than on the other. Instead of anæsthesia it is much more common to find a diminution or absence of reaction to impressions that normally should cause pain, *i. e.*, hypalgia or analgesia,—while at the same time the sensation of contact of a body and the reflex reactions of cough, gagging, etc., are only very little impaired. Sometimes pain is felt, spontaneously and in response to external impressions, at the same time that both tactile and reflex sensibility are diminished or lost, a condition which is called anæsthesia dolorosa. Sometimes the reflex re-action is very feeble, while contact and pain are normally felt ; while at other times the reflex sensibility is intact or even exalted, and all other sensitiveness lessened. In one case I noticed, in a very intelligent and otherwise healthy female patient of about 34 years, the curious phenomenon of delay of appreciation, *i. e.*, several seconds elapsed before reaction occurred.

As a constant symptom, objective always, and frequently also subjective, *i. e.*, when the patient becomes conscious of it, I call attention to the accumulation of saliva and phlegm in the throat. This is sometimes in the valleculæ, sometimes in the pyriform sinuses, or in both. I do not remember to have seen a well-marked case of hypæsthesia in which this symptom was not present to some extent ; and I call special attention to it because, when the patient is not aware of it, and the accumulation is not very abundant, it is apt to be overlooked. Hypæsthesia is accompanied with difficulty of swallowing and “wrong swallowing,” or food passing into the wind-pipe ; but this is due less to the in-

sensible condition of the parts than to associated muscular paralysis.

2. *Hyperæsthesia*.—The term hyperæsthesia is frequently used as though it were synonymous with hyperalgia, *i. e.*, increased painfulness. Bristowe,¹ while recognizing the proper meaning of the term, justifies this use because, as he says, “practically exalted sensibility is scarcely, if ever, distinct from painful sensibility.” As the rule, this is unquestionably true, although each of the three kinds of sensibility is liable to be exalted alone, or together with one or both of the others. Hyperæsthesia may vary in different cases in degree and extent, in the same manner as hypæsthesia.

When the tactile sensitiveness alone or together with the dolorous is increased, the name “*oxyæsthesia*,” meaning simply very sharp or acute sensibility, has sometimes been given to it. I have had a case of a gentleman who could distinguish—and painfully so—the points of my æsthesiometer two millimetres apart in almost every portion of his throat. Hyperæsthesia is sometimes so great, that not only contact but even approximation of a body is sufficient to produce reaction. This ideal reaction—especially reflex—occurs in cases of idiosyncrasy more often than in acquired hyperæsthesia.

When the dolorous sensibility is affected the pain is felt sometimes only when the parts are moved, as during swallowing, speaking, etc.; sometimes spontaneously as well. I have had several cases in which the pain more or less completely interfered with the use of the voice, producing what Coën, of Vienna, first described as phonophobia: in one case, even the slightest whisper caused agonizing pain. Spontaneous pain is sometimes continuous but more usually intermittent or at least remittent. When periodic, it frequently returns at the same

¹ “Theory and Practice of Medicine,” London, 1876, p. 932.

time of the day. It is always made worse by strong or sudden emotions, and sometimes disappears and reappears without assignable cause. True neuralgia unquestionably occurs, although the diagnosis in some of the cases reported as such may be questioned. The pains, usually in the front part of the throat and neck, are paroxysmal and flash with momentary intensity along the course of the nerves. Sometimes they are also felt in the back of the throat and in the neighborhood of the tonsils, and sometimes, though more rarely, extended peripherally as well as along the course of nerve fibres.

Occasionally the pains radiate from the throat as a centre to the neck, back, head, shoulders, arms, etc. The symptoms of reflex hyperæsthesia as ordinarily seen consist mainly of muscular contractions, such as retching, gagging, nausea (to vomiting), cough, expiratory spasm, inspiratory spasm, etc. I cannot here take these up in detail but would say that one or several of these may be present, while, at the same time, others and hyperæsthesia of another kind of sensibility, are absent. This is frequently the case in so-called "nervous laryngeal cough," which is a particular phenomenon of reflex hyperæsthesia. The circulatory and secretory effects are to some extent more recondite symptoms of reflex hyperæsthesia. The occurrence of lachrymal and nasal hypersecretion, as well as abundant flow of saliva and phlegm in response to impressions limited to the throat, is of course well known, but the reaction does not end here. No laryngoscopist can have failed to have noticed that perfectly healthy vocal bands are sometimes suddenly temporarily flushed and as suddenly paled, or that the mucous follicles in some part of the throat under his eye momentarily secrete from over-sensitiveness alone.

3. *Paræsthesia*.—Positive symptoms of paræsthesia are entirely subjective. Patients complain of perverted sensa-

tions, which may be referred to one or the other of the three kinds of sensibility, alone or in combination: there may be the spontaneous sensation of itching or scratching; of so-called formication or vermination; of heat, *i. e.*, burning, or cold; of dryness, of weight or pressure, of roughness, or rawness. Generally there is a sensation of the presence of some kind of a foreign body, either stationary or moving about, which may be a hair, a fish-bone, a burr, artificial teeth, a fly crawling about, or something else, which the patient believes has entered his throat. Sometimes there is a sensation of vacuity, of fulness, of stiffness, etc.

The three main classes of perverted feelings are, as I have already said, sphæræsthesia, zonæsthesia or strangalæsthesia, and akanthæsthesia, *i. e.*, of a ball- or globe-shaped body, of a cincture or halter, and of a sharp point. The first gives the feeling of fulness, the second of constriction, and the third of piercing. It is impossible to enumerate all the varieties of these classes. The symptoms exceptionally persist uninterruptedly day and night, interfering with sleep; sometimes they are absent for an hour, or a day, or several days, and then return. There is objectively found in cases of paræsthesia occasionally more or less hypæsthesia, but far more frequently hyperæsthesia. In these cases all sorts of pains are described—stinging, cutting, boring, crushing, etc. I am inclined to mention here, as an instance of perverted sensation, the case of a young lady with chronic hypertrophic catarrh, who positively experienced pleasure from applications to her naso-pharynx, which ought to have (*i. e.*, in the vast majority of cases in my experience, decidedly have) given considerable pain. I have, however, counted neither this case nor a few more or less similar cases among the 58 enumerated in this paper.

Diagnosis.—From the symptoms and, in appropriate cases, the æsthesiometrical examination, a diagnosis can easily be

arrived at, provided it be certain that there is no structural change present. This latter point can of course not be ascertained without a thorough pharyngo-laryngoscopical examination. *While, on the one hand, great care must be taken not to overlook local lesions,—and special attention should be given to the comparatively less accessible portions of the throat,—on the other, the mistakes must be avoided to regard as such local lesions the reflex effects of disordered sensibility, and to give undue prominence to consequential, unessential, and unrelated local complications.* Errors of diagnosis in all these directions have come under my observation, and were I not forced to content myself in this paper with this passing mention, I should like to refer to the subjects of elongated uvula, the actual presence of a foreign body, or small out-of-the-way ulcer, etc.

Frequent “wrong swallowing”—the tendency of food to pass into the larynx—in the absence of any obstruction to the entrance of food into the stomach, justly makes us suspect hypæsthesia of the throat. Under these circumstances an accumulation of saliva and phlegm strengthens that suspicion; but æsthesiometrical examination alone proves it to be true. Whenever the throat is complained of, especially when swallowing and the use of the voice, either or both, are attended with pain or any unpleasant sensation, in the absence of a local lesion to account for the symptoms, we must suspect hyperæsthesia or paræsthesia. In all such cases æsthesiometrical examination must be instituted. I have already described the manner in which the three kinds of sensibility are examined by means of my æsthesiometer. In the absence of this instrument, laryngeal probes, blunt-pointed and sharp, will answer the purpose except for the differential determination of the sensitiveness to temperature. In determining whether the sensibility is diminished or increased at any point, if one-

sided, we must compare it with what is known as the healthy side, and always with other portions of the throat in relation to the order of acuteness of sensibility which I have presented as a physiological basis, remembering, however, that the list given is a provisional and imperfect one. Unless difference of sensitiveness in one direction or the other is very considerable, we must be cautious in trusting to it. I have pierced the velum with the sharp point of the æsthesiometer, so that it bled, without the patient's feeling more than a contact; and Jurasz has reported a case of diphtheritic anæsthesia in which puncturing the posterior palatine arch on the right side, causing bleeding, was not even felt at all. It is so easy to graduate the strength of the electrical current in most Faradaic apparatuses, that I avail myself, by means of throat electrodes, of this current for determining the point at which the patient experiences pain or any sensation at all,—comparing the strength with that which is appreciated in other portions of the throat or other mucous membranes. On applying the galvanic current, some sensitive points are sometimes found (distinct from Valleix's painful points in the course of nerves in neuralgia) to which Fraenkel has called attention.

The general condition of the patient, the diseases which complicate disordered sensations, and the peculiar character of the symptoms in disappearing and returning and being influenced by psychical circumstances, help to determine the neurotic element in the case.

SECTION VII.—PROGNOSIS.

The prognosis should in all cases be cautiously framed. Sometimes the disorder is very grave. Generally the duration is uncertain, but palliation possible, even if cure is unattainable. Frequently, treatment, though difficult, is successful.

The danger of hypæsthesia comes from "wrong swallowing." A bolus may suffocate the patient, or fatal so-called "food pneumonia" may result from the entrance of food into the air-passages. The danger in hyperæsthesia comes from difficulty of swallowing and from the effects of the irritation of the reflexes upon the general health. In paræsthesia the psychical effects have a tendency to undermine health.

SECTION VIII.—TREATMENT.

The patient's general health should, in all cases of disordered sensation, receive attention; and occasionally nothing more need be prescribed than change of air, traveling, a course of hydropathic treatment, or tonics and alteratives. The proper psychical treatment is also of importance. Always in hypæsthesia, and sometimes in hyperæsthesia and paræsthesia, quinine internally, and by insufflation locally, is especially useful. I have cured recent and slight cases by an emetic. Locally, I have found the frequent use of my "throat educator"—originally devised to overcome the hyperæsthesia interfering with laryngoscopical examinations, and consisting simply of a smooth piece of hard rubber or wood—of more or less benefit in nearly every case of sensory neurosis. Any other instrumental appliance, or, perhaps, the finger, might do as well, but it must not be forgotten that a great deal depends in these neurotic cases upon psychical impression. Sprays of pure and medicated water, either hot or alternately hot and cold, forced into the throat with a pressure of from five to twenty-five pounds; applications, by sponge and brush, of a saturated solution of iodoform in sulphuric ether; and, finally, electricity, induced and dynamic, have served me good purposes in the most varying cases of these disordered sensations.

In addition :

In hypæsthesia I have used internally, phosphorus, and nux vomica,—strychnine, also, by insufflation, and hypodermically (metallo-therapy and quite recently xylotherapy have been recommended); and, if, in swallowing, food enters the larynx, the patient must of course be fed by means of an œsophageal tube.

In hyperæsthesia we have, in the bromides, especially potassium bromide, both internally and locally, and in morphine, dissolved in mucilage, used locally, and hypodermically to produce its general anodyne effects,—agents which temporarily, and, sometimes, permanently, control the disordered sensations. Neuralgic pains are sometimes temporarily relieved by aconitine ointment, externally applied, sometimes by equal parts of camphor, chloral, and chloroform, externally, and sometimes even by ether spray externally. Internally, three-grain doses of mono-bromide of camphor every few hours, or fifteen minims of hydro-bromic acid in water every four hours, have been strongly recommended.

In paræsthesia, besides the general and special treatment already indicated, I have seen occasional good effects from zinc phosphide, zinc cyanide, belladonna, and ergot.

In conclusion I would say, that whenever there are present, in cases of dysæsthesia, any local lesions, they should of course be treated, and if possible cured. Their continuance does sometimes keep up the disordered sensation, although their removal is not always followed by its cessation.

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RATTLESNAKE VIRUS: ITS RELATIONS TO ALCOHOL, AMMONIA, AND DIGITALIS.

BY ISAAC OTT, M. D.

THE snake poisons have been the subject of but little investigation except by Drs. S. Weir Mitchell and Fayrer. This is somewhat surprising, considering the importance of the subject to every man travelling in country districts. I have mapped out a series of researches upon all the poisonous snakes procurable, with the view of studying the physiological action of their special poison and the means of counteracting its effects. In this paper I intend to confine myself to the action of the poison on the circulation and the agents which are powerful in sustaining it. The circulatory changes were studied by means of Ludwig's kymographion, so that during each second I could observe the counteracting effect of the drugs named. The snake was enclosed in a box which could be separated into two compartments by a sliding door. The snake was put into one apartment and the rabbit or his leg into the other, when the sliding partition was raised and the snake allowed to strike. Then the kymographion was attached as soon as possible, and the changes noted or the drug injected. I shall first give the general action of the poison of the rattlesnake upon the circulation.

Exp. 1.—Rabbit struck on the foot by the snake.

TIME.	PULSE.	PRESSURE.	
10.15. 0 A. M.,			struck by snake.
10.15.15 "	83	81	
10.15.30 "	90	81	
10.16.30 "	79	78	
10.17.30 "	75	50	
10.18.30 "	80	72	
10.21.30 "	81	68	
10.23.30 "	82	68	
10.36.30 "	82	72	
10.50.30 "	80	69	
11.20.30 "	death ; chest opened ; heart beating slightly.		

Exp. 2.—A little rabbit was struck by the snake, after he had rested four days, at 11.50 A. M. Defecation and urination ensued. Animal sits perfectly still ; weak voluntary power over the extremities. 12.15 P. M., cardiac movements very feeble ; posterior extremities spread outward ; sinks down. 1 P. M., death ; heart making a few trembling, weak movements.

Exp. 3.—Large rabbit was struck by a snake after a rest of a week. The pulse was too weak to be registered ; chest opened ; heart making trembling, imperfect systoles. Ammonia, when driven into the lungs, did not rouse the heart or increase the pressure. Death took place by convulsions in about a half hour after the snake struck the animal. Right auricle beat three hours after death.

Exp. 4.—Rabbit was struck by snake, after he had rested about four days, at 9.56 A. M. Was struck in nose and at root of ear. 10 A. M., tremors in both posterior extremities. 10.6 A. M., respiration very quick, panting. 10.8 A. M., registration of pulse began.

TIME.	PULSE.	PRESSURE.	
9.56. 0 A. M.,			snake struck animal.
10. 8.15 "	64	80	
10. 8.30 "	63	72	

TIME.	PULSE.	PRESSURE.
10. 9.15 A. M.,	58	60
10.10.30 "	59	68
10.11.45 "	58	66
10.13. 0 "	58	62
10.14.15 "	60	68
10.15.30 "	59	65
10.16.45 "	62	74
10.18. 0 "	62	72
10.19.15 "	66	70
10.19.30 "	62	68
10.20.30 "	64	70
10.21.30 "	65	68
10.22. 0 "	60	66
10.29. 0 "	animal died.	

The pulse and pressure, after the injection of the virus subcutaneously, gradually fall, especially the pressure, which decreases greatly. The fall of pulse is not very great till near death. In one experiment there was a momentary increase of pulse before the fall, whilst after the cessation of breathing, the heart beats in a trembling manner for a short time and then lies in a relaxed, flattened condition. Yet I believe that the main cause of the arrest of respiration is mainly due to the weakened systoles and low pressure. Occasionally when the heart had stopped electrical stimuli would not excite it. The independence of the respiratory centre is greatly interfered with by cardiac weakness, which I believe to be the main cause of death in the cases here noted.

That the snake may kill in a few minutes is quite true, and the cause here may be due to other coöperating influences in a large part. In the study of the antagonism of drugs to the rattlesnake poison, it must be remembered that rabbits are usually killed by it, whilst the dog, according to Dr. Mitchell, rarely dies, so that the drug must be quite effective to prevent death in the animals experi-

mented upon. It must also be noted that the snake had been used on an average every week or two for an experiment for four months. To study the antagonism of the above-named drugs they were injected through the jugular toward the heart, when the pulse and arterial tension were being registered. I shall give examples with liquor ammonia.

Exp. 5.—Rabbit struck by a snake after he had rested ten days.

TIME.	PULSE.	PRESSURE.	
2.42. 0 P. M.,			snake struck animal
2.50. 0 "	60	34	
2.50.15 "	53	36	$\frac{1}{2}$ gtt. of ammonia in 1. 5 c. c. of water.
2.50.30 "	43	48	
2.50.45 "	56	54	
2.51. 0 "	58	50	
2.51.15 "	57	40	
2.51.30 "	60	52	
2.52. 0 "	65	56	
2.52.15 "	67	54	
2.52.30 "	71	46	
2.54.30 "	69	44	
2.54.30 "	70	40	
2.55.30 "	67	40	
2.57. 0 "	50	24	$\frac{1}{2}$ gtt. ammonia in 1. 5 c. c. of water.
2.57.15 "	53	34	
2.57.30 "	47	30	
2.57.45 "	60	24	
2.58.45 "	63	20	
3. 2.45 "	62	32	
3. 7.45 "	26	10	$\frac{1}{2}$ gtt. ammonia in 1. 5 c. c. of water.
3. 8. 0 "	24	4	
3. 9. 0 "	death.		

This and other experiments demonstrated that ammonia

temporarily increases the pulse and pressure in rattlesnake poisoning, but that finally its injection in an almost exhausted heart rapidly stops it.

The next drug tried was alcohol.

Exp. 6.—Rabbit struck by snake after a rest of four days, just before ecdysis.

TIME.	PULSE.	PRESSURE.	
I.23. 0 P. M.,			snake struck.
I.35. 0 "	76	40	
I.36. 0 "	80	36	Five minims of alcohol in 16 cubic centimetres of water slowly introduced.
I.36. 5 "			
I.36.15 "	60	64	
I.36.20 "	death.		

Exp. 7.—Rabbit, snake rested nineteen days; ecdysis going on, panting respiration; struck at 8.30 A. M.; 9 P. M. death, although ten minims of diluted alcohol were injected through the jugular five minutes before death; chest opened, heart making trembling movements.

Exp. 8.—Rabbit struck by snake after he had rested fourteen days.

TIME.	PULSE.	PRESSURE.	
4.40. 0 P. M.,			snake struck.
4.49.45 "	64	42	10 minims of alcohol in eight c.c. of water.
4.50. 0 "			
4.51. 0 "	59	54	
4.52. 0 "	61	54	
4.53. 0 "	74	46	
4.54. 0 "	74	48	
4.55. 0 "	56	50	
4.55.15 "	38	34	
4.55.30 "	animal dead.		

The experiments with alcohol show that it has the power to temporarily stay the falling arterial tension and the decrease of pulse.

I shall give an experiment with digitalis. The officinal infusion was used.

Exp. 9.—Large powerful rabbit was struck twice, once through the middle of the ear; snake had rested twenty-seven days.

TIME.	PULSE.	PRESSURE.	
12.27. 0 P. M.,			snake struck.
12.35. 0 "	79	82	
12.35.15 "	82	78	6 c. c. infus. digitalis.
12.35.30 "	72	83	
12.35.45 "	60	108	
12.36.15 "	59	116	
12.37.15 "	58	102	4 c. c. infus. digitalis.
12.42.15 "	58	70	
12.50.30 "	52	58	
12.53.30 "	55	50	
1. 8.30 "	62	76	
1.15.30 "	57	74	

Animal left up in a drooping condition till evening, when he was killed by a stab in the medulla.

Exp. 10.—Small rabbit struck by snake after a rest of six days.

TIME.	PULSE.	PRESSURE.	
2.0. 0 P. M.,			snake struck.
2.12. 0 "	70	75	
2.12.15 "	63	67	4 c. c. infus. digital.
2.12.30 "	63	94	
2.12.45 "	70	90	
2.13. 0 "	67	88	
2.13.15 "	68	78	
2.13.30 "	72	84	
2.13.45 "	68	80	
2.14. 0 "	70	84	

TIME.	PULSE.	PRESSURE.
2.14.15 P. M.,	70	78
2.15.15 "	67	72
2.16.15 "	too faint to count.	84
2.17.15 "	—	90
2.20.15 "	4 c. c. infus. digital.	
2.21. 0 "	death.	

All these experiments seemed to show that infus. digitalis had the property to stimulate the main failure, a falling arterial tension.

When alcohol and digitalis were combined the results were about the same.

Exp. 11. Rabbit struck by snake after a rest of four days, ecdysis having taken place two days previously, on Aug. 15, 1881.

TIME.	PULSE.	PRESSURE.	
1.20. 0 P. M.,			snake struck.
1.28. 0 "	65	50	
1.29. 0 "	68	50	
1.30. 0 "	71	48	
1.31. 0 "	68	46	
1.32. 0 "	67	46	
1.33. 0 "	66	44	
	1 c. c. tr. digitalis in 4 c. c. of water.		
1.33.15 "	59	50	
1.33.30 "	59	50	
1.34.30 "	67	52	
1.35.30 "	74	102	
1.36.15 "	Pulse very feeble, one half c. c. of tinct. digitalis in 3 c. c. of water when pulse fell rapidly.		
1.38. 0 "	death.		

The following conclusions may be drawn from the experiments:

1. The rattlesnake poison mainly kills by producing a failure of the cardiac organ, and a great fall in arterial tension.

2. That ammonia, alcohol, and digitalis, temporarily increase the arterial tension.

3. That ammonia and alcohol increase the rate of pulse whilst digitalis slows it.

4. That toward the close of life the intravenous injection of either alcohol, ammonia, or digitalis stimulates the circulatory apparatus, but the excessive stimulation totally and rapidly exhausts the cardiac irritability. I purpose shortly to use the above agents first and then allow the snake to bite the animal, when I will consider the relation of the drugs more fully.

SUICIDE IN EUROPEAN AND AMERICAN CITIES.*

By ALBERT LEFFINGWELL, M.D.

“S UICIDE,” says Goethe, “is an event of human nature which, whatever may be said or done with respect to it, demands the sympathy of every man, and in every epoch must be discussed anew.” It is the paradox of conduct, the search for greater happiness by means which it is the very instinct of human nature most to dread. In every age it has furnished a text for religion, a theme for the dramatist, a subject for philosophy; but it is only within the present century that science has been able, by means of accurately gathered statistics, to measure its prevalence, to ascertain somewhat of the laws which govern its production, and to point out the methods by which we may hope to check its threatening tendencies. We may now know not merely the number of suicides which occur every year in each European capital or country; but we can distinguish the number of either sex, the causes which directly or indirectly impelled, the periods of the year, the presence or absence of family ties, the very methods they employed, and even the ages at which they grew tired of living and sought release.

To the American student, however, it is unsatisfactory to

* Skene prize essay, read at the Annual Meeting of the Alumni Association of the Long Island College Hospital, June, 1881.

know facts of this character only with regard to Europe; he desires also to know how the love of life is affected by our Western civilization, under a government where all men are peers. "How," he asks, "does the rate of suicide in American cities contrast with its prevalence abroad? Is the annual rate of suicide in Brooklyn, Philadelphia, or New York greater or less than in London or Paris, Vienna or Berlin? Do influences of sex and race linger under the changed environment of the New World? Are the same methods in vogue in American cities as in Europe, and may we see any possibility of raising barriers against self-murder through legislation on the sale of poisons? Is the age of greatest tendency to suicide the same? Do family ties exert here the same influence? Does education impede or religion restrain?" These are questions to which a satisfactory reply is impossible from existing literature, nor to be had at less cost than a thorough personal investigation of the whole subject. This, the present writer several months ago determined to undertake. The labor has been far greater than he anticipated. Alone among civilized nations the United States has no exact record of its births, deaths, or marriages from year to year; nothing, as a nation, but the carelessly gathered facts of the census-taker collected once in ten years. Almost the only exception to this utterly unscientific carelessness are the health departments of a few cities. A satisfactory comparison of metropolitan suicide in Europe and America would have been absolutely impossible but for the generous assistance of members of the medical profession connected with these boards. From nearly every capital city in Europe to which inquiries were sent information was returned. Dr. Bertillon, "chef de la statistique de la Ville de Paris," and the most eminent statist in Europe: the Registrar-General of England; Dr. Berg, chief of the Statistical De-

partment of Sweden; Dr. Maurice Gad, of Denmark; Dr. Josephy, of Vienna; and Dr. Engel, of Berlin,—all these responded most generously to requests for detailed information in regard to suicides of their respective countries or capitals. To the health departments of several American cities (and especially to the registrars of vital statistics of Brooklyn and New York) the writer is under peculiar obligations. In the present essay we shall attempt to classify and contrast only a part of the information thus obtained.

FREQUENCY OF SUICIDE.

In order to obtain a fair conception of the prevalence of suicide, it will be necessary to view its occurrence, not for a single year, but for a period sufficiently extended to show that the numbers are due to no extraordinary combination of circumstances, but are of *regular occurrence*. It is needless to furnish a detailed abstract for all the cities we shall examine, and in the following table I have selected Brooklyn and New York, London and Berlin; and give herewith the suicides of either sex for a period of ten years.

SUICIDES, FOR A PERIOD OF TEN YEARS, OF EITHER SEX.

	BROOKLYN.		NEW YORK.		LONDON.		BERLIN.	
	Men.	Women.	Men.	Women.	Men.	Women.	Men.	Women.
1869	—	—	—	—	229	78	—	—
1870	—	—	79	22	196	85	116	56
1871	18	10	85	29	207	86	119	29
1872	23	8	111	33	196	74	101	39
1873	22	9	82	30	211	66	115	52
1874	30	4	140	40	189	68	156	48
1875	29	10	123	32	199	98	166	46
1876	41	10	114	36	221	74	183	65
1877	35	15	123	25	202	70	231	73
1878	41	11	116	26	225	93	251	72
1879	30	7	100	17	—	—	251	107
1880	26	5	—	—	—	—	—	—
Total by sex .	295	89	1,073	296	2,075	792	1,689	587
Both sexes .	384		1,369		2,867		2,276	

We cannot help experiencing a feeling of wonder in the study of statistics like these,—not only at the average regularity with which men and women of such different surroundings fling away their lives from year to year, but also at the remarkable and uniform difference in this respect manifested by persons of different sex. No matter how widely, in the countries of Europe, the variance in the rate of suicide may be (and it is no less than thirteen times greater every year in Denmark than in Ireland), the proportion between the sexes is always about the same. Take the four cities we have just examined in detail; and of the total suicides for ten years, between 70 and 80 per cent. will always be men.

OF TOTAL SUICIDES, WHAT PER CENT. ARE MALES OR FEMALES?

	Male.	Female.	Both.
New York, 1870-'79	78	22	100
Brooklyn, 1871-'80	77	23	100
Berlin, 1870-'79	74	26	100
London, 1869-'78	72	28	100

A much better method of looking at this phenomenon is by comparing the proportion which suicides of either sex bear to the male and female population in the cities where they occur, and in the following table I have estimated this proportion for five European capitals and as many American cities.

My comments on this table must be necessarily briefer than its importance deserves. Several points should be noted by the reader. The first question naturally arising is, what degree of reliance can be placed upon these deductions?

TO 1,000,000 LIVING OF EACH SEX, HOW MANY SUICIDES ANNUALLY?

Period observed.	Date of Census, or Estimate of Population.	CITY.	AVERAGE ANNUAL NO. OF SUICIDES.		AVERAGE NO. OF SUICIDES ANNUALLY TO 1,000,000 LIVING.	
			Men.	Women.	Men.	Women.
1877, '78, '79 (3)	1876	Paris,	555	171	551	175
1870-'79 (10)	1875	Berlin,	169	58	349	121
1869-'78 (10)	1878	Stockholm,	37	8	439	91
1869-'78 (10)	1875	Vienna,	123	42	241	82
1869-'78 (10)	1872	London,	207	79	131	45
1878, '79, '80 (3)	U. S., 1880, $\frac{1}{2}$ and $\frac{1}{2}$.	} San Francisco,	74	11	699	110
1870-'79 (10)	State census, 1875.	} New York,	107	29	211	55
1871-'80 (10)	Estimate of 1875.	} Boston,	27	7	186	42
1871-'80 (10)	State census, 1875.	} Brooklyn,	29	9	127	35
1869-'78 (10)	Estimate of 1875.	} Philadelphia,	40	9	126	25

For Europe, they are as nearly accurate as possible. For the period observed, I have in all cases taken the census figures of population, when to be obtained, for the year nearest the middle of observed period. For Paris, the rate is probably a little higher than it should be, because estimated by the census of 1876, instead of an estimate of population for 1878. For Stockholm, for a similar reason, the rate is a little lower than it should be; say for both cities three or four units—too inconsiderable to affect general reliability.

San Francisco presents such startling and exceptional results that I have taken only three years and estimated

the number of suicides by the population in 1880, making even the high rate obtained less than it should be. New York and Brooklyn are very accurately measured by the State census. For Philadelphia and Boston I have taken the mean of the two U. S. censuses of 1870-1880 to be the probable population in 1875. Let us now see what deductions may be safely inferred.

Suicide, when measured by population, is not only invariably three to five times more frequent among men than among women, in Europe and America, but the causes which, in one city, increase its prevalence among men, heighten the rate proportionately (or nearly so) among the other sex. This is so true, that we could even calculate very nearly what the rate of suicide would be, under similar state of civilization and environment, were knowledge wanting. Suppose, for example, that we knew nothing whatever of the rate of suicide among the female population of Brooklyn, would it be possible to estimate it with any degree of accuracy? Certainly, if we know the facts for New York. The two cities lie side by side, and if any causes have tended to make the rate of male suicide lower in the City of Churches, we might be sure that they have proportionately influenced the female population. The problem would be an exceedingly simple one:

RATE OF SUICIDE FOR MEN.

N. Y. *Brooklyn.*

211 : 127

RATE OF SUICIDE FOR WOMEN.

N. Y. *Brooklyn.*

55 ?

and we should find our answer to be 33—only two less per million population than the actual statistical evidence of ten years. Why fewer women than men commit suicide is one of the questions to which reply cannot be satisfactorily given. It may be that everywhere, especially in our great cities, fewer women than men are subjected to the “slings

and arrows of outrageous fortune"; that upon her falls less frequently the poverty which disheartens, the keen anxieties and disappointments of commercial life, the temptations of intemperance, or the incitements to vice and crime. It may be that, on that lower level of destitution and misery, where man can see no refuge from beggary but in suicide, woman perceives too often a door flung open, and hands stretched out to welcome her, to longer but dishonored years. Or, perhaps, and more probable, it is because her moral nature is inherently stronger and better than ours that woman hesitates longer before temptation, that the promises of religion are more highly valued, and its restraining influences more surely effectual.

INFLUENCE OF RACE.

We see, too, that the rate of suicide in every European capital named is higher than in Brooklyn. In Paris the rate of suicide is nearly *five* times, and in Berlin almost *three* times higher for both sexes than with us. Even New York is exceeded by every city except London, whose rate of suicide very closely approximates that of Brooklyn. But even this exhibit is not as clear as we would like. If it be true that the suicidal tendency of certain races is very marked, the presence in our midst of a large element, foreign-born, might naturally be expected to do much in heightening our rate.

Now I am indebted to Dr. Wyckoff, of Brooklyn, and Dr. Nagle, of New York, for a record of the nationality, so far as ascertained, of all suicides in Brooklyn and New York, for a period of ten years. And during this time there is not a single year in New York (and but one in Brooklyn) in which the foreign-born suicides do not outnumber the native-born of either sex. The total results may be of interest:

NATIONALITY OF SUICIDES, BROOKLYN AND NEW YORK.

(So far as ascertained.)

	1871-'80. BROOKLYN.		1870-'79. NEW YORK.	
	Men.	Women.	Men.	Women.
Born in U. S. (10 years) .	97	24	254	78
Foreign-born " " .	198	65	819	218

But what is the proportion between these numbers and the native- and foreign-born population from which they arise? That is the question we need to answer, and the reply to which is somewhat difficult. The usual method of calculating ratios of this kind is by dividing the number produced by the population which produces; yet if we should do this, and should attempt to calculate rate of suicide to total population, native- or foreign-born, it is evident we should have reckoned among the "native-born" thousands of children—one, two, and three years of age, and so on—whose parents were foreign-born, and who ought, in justice, to be included with them or subtracted altogether. Suppose we do that for New York, and subtracting from the population all under 15, ascertain the ratio of suicide to the population thus remaining. The result would be as follows:

NEW YORK CITY.	BORN IN THIS COUNTRY.		FOREIGN-BORN.	
	Men.	Women.	Men.	Women.
Population over 15 in 1875. .	132,376	141,031	201,626	220,473
No. of suicides, 1874-'75-'76.	83	28	294	80
Average <i>annual</i> rate of suicides to 1,000,000 living— over age of 15 . . . }	208	66	486	121

If these results may be accepted, it would seem that the tendency to suicide is about twice as strong with men and women of foreign birth as for native-born Americans. Yet

even here distinctions must be drawn. The rate of suicide among the English, Scotch, and Irish is even less than among Americans; and the high rate shown by the foreign portion of our city population is almost wholly due to the Teutonic and Scandinavian element composing it. Other questions press for solutions, as yet impossible to give. It may be that this higher rate of suicide with foreigners is partly due to more poverty and greater destitution; more of the circumstances which dishearten and tend to create despair. Perhaps far more found themselves deceived in their dreams of obtaining wealth or even a livelihood in our great metropolis; more were homeless, penniless, friendless, among strangers, and without the means of support. Still, after deducting all these contingencies, there can be no doubt that *race* is an important factor in the prevalence of suicide. Upon no other hypothesis can we explain the differences between Berlin and Stockholm, between Paris and London.

INFLUENCE OF AGE.

At what time of life do most suicides occur? Here, again, statistics vary somewhat according to race and sex, and other phases of environment. But no age is entirely exempt; the school-boy of less than a dozen years, and the aged pensioner of fourscore, have taken into their own hands the settlement of the question of continued existence. If we take the total number of suicides in London, Paris, and New York, and ascertain the rate per cent. which the suicides at given ages bear to the total for all periods of life, we shall get the result tabulated below. For Paris the official reports do not distinguish the sex of suicides as regards age, and we can show these differences only in the last columns.

WHAT PER CENT. OF SUICIDES ARE AT VARIOUS AGES ?

AGE.	MEN.		WOMEN.		BOTH SEXES.		
	New York.	London.	N. Y.	London.	N. Y.	London.	Paris.
15 to 35	35%	26	44	36	37	29	30
35 to 45	27	22	22	21	26	22	19
45 to 55	20	22	21	19	20	21	21
55 to 75	17	27½	11	22½	16	26	28
75 and over.	1	2½	2	1½	1	2	2
All ages.	100	100	100	100	100	100	100

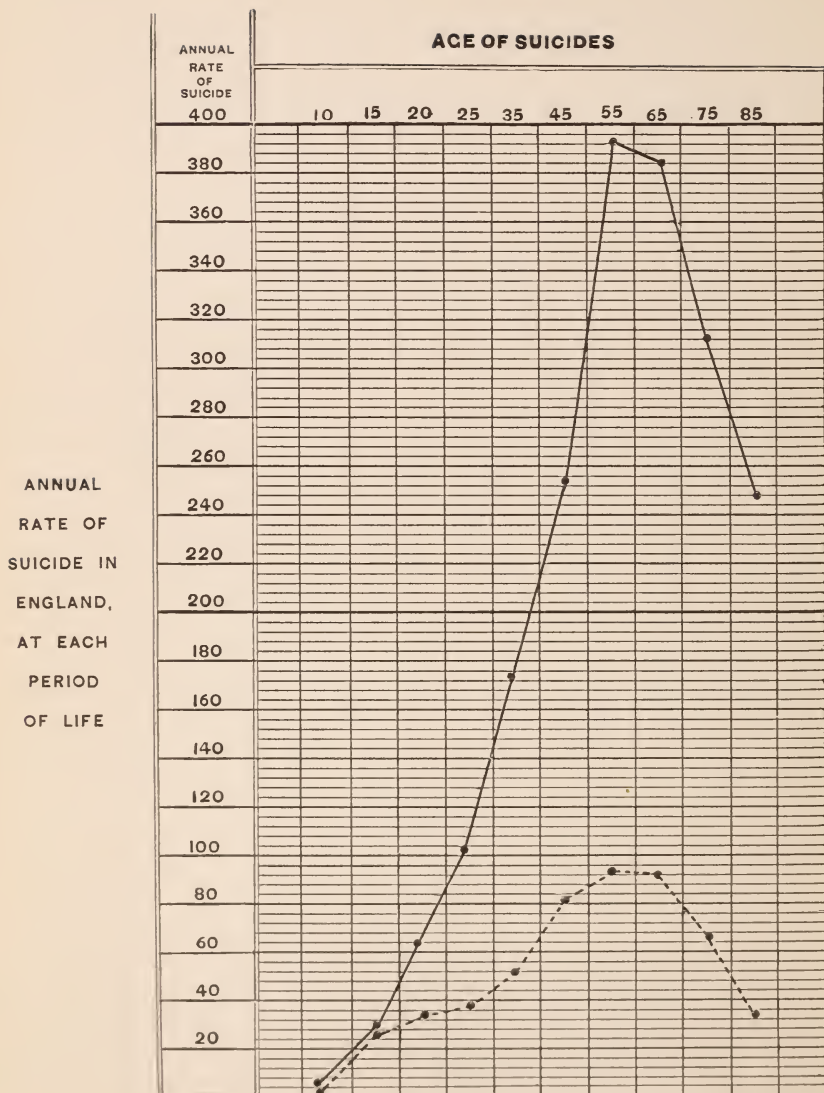
This is a curious table. At only one period of life, between 45 and 55, is there any substantial agreement between these three cities. Yet between London and Paris there is far less difference than between either city and New York, as respects age of a majority of suicides. Do we, then, in America tire of living sooner than the Frenchman or Englishman? It would almost seem so, judging from the ages at which the higher percentages of suicides occur. In New York over 62 per cent. of suicides are under 45 years of age ; while in Paris or London, the proportion is only one half. We can see, too, from this table that the impulse to suicide is proportionately far greater in youth with women than with men ; and it is doubtless because the influence of disappointment in her affections is far stronger with the weaker sex.

Still this is an unsatisfactory comparison ; what we wish to ascertain is the proportion which the suicides at each period of life bear to the number living at the same age. But for cities, with their ever shifting and transient population, this is a very difficult, if not (at least for America) an

impossible task. If, however, we obtain the same facts for a nation, we shall obtain at least a clew to the facts as they probably exist in its cities and towns. I have, therefore, at considerable expense of time, taken the reports of the Registrar-General of England for ten years, 1867-1876, and ascertained the number of suicides at each recorded age, both male and female; and estimated what proportion the average annual number of these self-murders bears to the population living in 1871 according to the English census of that year. The results are sufficiently interesting to be included in the present study. To the final columns, however (in heavy faced type), the reader's attention is specially directed.

TO 1,000,000 LIVING AT DIFFERENT AGES, WHAT IS THE RATE OF SUICIDE, ANNUALLY, IN ENGLAND AND WALES?

MEN.				WOMEN.			
AGE.	Population at different ages, by census of 1871.	Number of suicides 1867-1876.	Rate per annum to 1,000,000 living.	Population of different ages, census of 1871.	Number of suicides 1867-76.	Rate per annum to 1,000,000 pop.	Proportion of male and female. Rates of suicide to each other at each separate age.
10 . .	1,224,544	50	4	1,207,224	40	3	58 to 42
15 . .	1,079,775	294	27	1,109,854	294	26	51 to 49
20 . .	963,152	594	62	1,045,391	334	32	64 to 36
25 . .	1,594,513	1,593	100	1,756,437	643	37	73 to 27
35 . .	1,234,721	2,157	175	1,344,420	719	54	76 to 24
45 . .	965,711	2,456	254	1,038,224	840	81	76 to 24
55 . .	642,562	2,513	391	702,456	655	93	81 to 19
65 . .	356,357	1,375	385	411,233	378	92	81 to 19
75 . .	121,037	378	312	151,633	98	65	83 to 17
85 and over.	14,544	36	247	23,280	8	34	88 to 12
	8,196,914	11,446	Average 140	8,790,152	4,010	Average 46	



Now if we cast these results into the form of a diagram, representing to the eye the prevalence of suicide by age and sex, we may bring out the facts even more vividly than by tables. See plate, fig. 1.

We reach, therefore, the following conclusions respecting age in its relations to suicide :

1. Between ten and twenty years the rate of suicide is nearly equal with both sexes.

2. With both sexes (although in widely different ratios) it reaches its maximum between fifty-five and sixty-five; from sixty-five, steadily declines, remaining nevertheless, even at eighty-five and after, higher than in youth. A recent contributor to *Blackwood's*, writing upon suicide, says that "the number of suicides in proportion to population grows steadily through all periods of life, from childhood to old age. People go on killing themselves between nine and ninety in a constantly increasing progression." This, we see, is a mistake; the limit is reached before ninety, although the facts are sad enough as they are. They indicate that suicide is not principally the resort of the thoughtless, the weak-minded, the impulsive, or the lunatic; but that it is, as another writer affirms it to be, "the refuge mainly of those who are worn out in a bitter and hopeless struggle against accumulated ills."

3. No age, except that of infancy, is entirely exempt. Among the suicides in England during the ten years examined I discovered one case of a girl of five years! Dr. Allan McL. Hamilton [Johnson's *Cyclopedia*, art. "Suicide"] states that "the common time of life for this crime is the period between the twentieth and fortieth year; although cases have been known to occur among young children, and at the age of seventy." The impression thus given, that suicides at the age of seventy are somewhat phenomenal, is by no means correct. If the reader will turn to page 152

he will see that in England, during the ten years 1867-76, no less than 414 men and 106 women committed suicide after the age of seventy-five; while the annual average of suicides over eighty-five is between three and four! If we turn to cities we find that in New York there were, during 1870-79, 13 cases of suicide after seventy-five; that in London (1867-76) there were 59 suicides after that age, and in Paris, during only three years (1877-79), there were no less than 113 instances over seventy, of which 29 were between seventy-five and eighty, 8 between eighty and eighty-five, 4 between eighty-five and ninety, and one poor wretch, who refused to be longer neglected by death, and committed suicide between ninety and ninety-five!

INFLUENCE OF MARRIAGE.

We come now to an exceedingly interesting problem in our study. What effect does the presence or absence of marital responsibilities exert upon the tendency to self-destruction? Considering suicide as the expression of man's inexpressible distaste for longer existence, it is certainly worth while to know whether the felicities of marriage strengthen his attachment to life; or whether its cares and anxieties, and the unhappiness which, unfortunately, sometimes accompanies it, do not, on the whole, overbalance its restraining influences. In seeking the solution of this question it seemed to me better to go over the ground myself, rather than rest upon authorities; and letters were therefore addressed to the principal statisticians of Europe, asking returns of social condition of all suicides in their respective countries or cities. Curiously enough, English statistics, otherwise so valuable, could give no light whatever on this question; the Registrar-General, Sir George Graham, wrote me that not even for London could this information be obtained. From other sources, fortunately,

facts were gathered; and in the following table I am able to present the social condition of suicides, not only for three European capitals and as many countries, but also for ten years in New York, four years for Brooklyn, and three for San Francisco.

SOCIAL CONDITION OF SUICIDES COMPARED.

Period Observed.	Country or City.	SOCIAL CONDITION SO FAR AS KNOWN.			OF EACH 100, SUICIDES PER CENT.		
		Married.	Single.	Widowed.	Married.	Single.	Widowed.
1863-'75	France . .	28,677	21,766	10,616	47.	35.⁶	17.⁴
1869-'78	Sweden . .	1,832	1,410	469	49.⁴	38.	12.⁶
1870-'79	Denmark . .	2,432	1,388	947	51.	29.	20.
1869-'78	Stockholm . .	139	310		31.	69.	
1869-'78	Vienna . .	519	864	136	34.	57.	9.
1870-'79	Berlin . .	870	968	268	41.³	46.	12.⁷
1877-'80	Brooklyn . .	112	36	16	68.	22.	10.
1870-'79	New York . .	681	396	127	56.⁶	33.	10.⁴
1878-'80	San Francisco	101	139	25	38.	52.⁵	9.⁴

We have seen that percentage estimates are simply an approximation to truth, satisfactory only because in so many cases nothing else can be had. It would be very unsafe to assume—as the reader might be inclined to do—that because 47 per cent. of French suicides were married, and only 35 per cent single, marriage therefore increases in place of lessening the suicidal predisposition. Dr. Bertillon, of Paris, has kindly furnished me with the proof that, so far from this being the case, an exactly opposite conclusion

must be drawn, when statistics of social population are taken into account.

FRANCE, 1863-1875.

Social State.	Average Annual Population of France, 1863-75. <i>Above legal age:</i> Men, 18; Women, 15.	Total Number Suicides.	Average Annual No. of Suicides.	Rate to 1,000,000. Living. <i>Each Status.</i>
Married .	15,088,500	28,677	2,206	146.⁸
Single .	8,180,325	21,766	1,674	204.⁷
Widowed	2,894,575	10,616	817	428.⁶

When we take population into our problem, we find, therefore, that bachelors and "spinsters" are far more prone to suicide than the married; and that the greatest predisposition is exhibited by widows and widowers. Similar results, Dr. Bertillon claims, have been obtained throughout Europe, wherever the statistics necessary for calculation have been at hand. Marriage, then, is more than a means of personal happiness; it is a benefit to society, an advantage to the State, to be encouraged in every way possible; a check to suicide and crime. One point, for which considerations of space forbid adducing proof, may be mentioned before leaving the subject. As a deterrent from suicide the influence of marriage seems to act upon women with far greater force than it exerts upon men. Perhaps this is only another proof that

" Man's love is of man's life, a thing apart;
'T is woman's whole existence."

Nevertheless it must be confessed that the statistics for New York and Brooklyn have a slightly suspicious look about them. Are there, then, so many more married persons in our stirring American cities as justify these proportions of suicide? In Brooklyn, for example, are there

above the age of 15, three times as many married persons as there are bachelors and maids, widows and widowers? Of the population of New York over 15, is only one third unmarried? At first thought it assuredly seems questionable whether the married population of our two cities is so much greater than its proportion in Berlin, Vienna, or Stockholm, and yet if it is not, then marriage fails to have with us that controlling influence which it exerts abroad. It is a problem for which the complete census returns will perhaps afford us means of solution at present denied.

One error, in an authority very frequently quoted, may be here pointed out. Dr. Forbes Winslow, in his "Anatomy of Suicide," says: "It has been satisfactorily established that among men two thirds who destroy themselves are bachelors." Now although, as I have shown, the tendency to suicide among bachelors is undoubtedly greater than among the married, there is no justification, so far as I can discover, for such a wide-sweeping statement as this. In Vienna, for example, the proportion of bachelors during ten years was a little over half the men, or 55 per cent.; in Berlin they were less than half, or 44 per cent.; in Sweden they were 38 per cent.; in Denmark but 30 per cent., or less than one third. Such statements as the one quoted make clear how wide from truth may be the highest authority when founded upon opinion instead of fact.

· INFLUENCE OF SEASONS.

There can be no doubt but that the different seasons of the year sensibly and persistently influence the propensity to self-destruction. Contrary to what might be supposed, it is not the melancholy days of autumn, nor the gloom of winter, but the brightness of spring and summer which exert this effect. I pass by the evidence of this fact in detail, to bring forward a very singular result in relation to

this phenomenon. If we divide the year into two halves,—spring and summer on the one hand, autumn and winter on the other,—we find that not only do suicides preponderate during the more genial seasons, but also occur in a majority of cases the initiatory symptoms of insanity—(if statistics of first admissions to asylums can be fairly taken as evidence on this point, as I think they can). Still more strange is the fact that if we take the criminal records of a country like France, and separate the crimes against property (such as larceny, theft, burglary, etc.) from crimes against persons (rapes, violent assaults, attacks with intent to kill, from anger or revenge, etc.), and of these last-named ascertain

INFLUENCE OF SEASONS UPON SUICIDE, INSANITY, AND CRIME.

	Per 1,000 cases, proportions happening during		
	Spring and Summer.	Autumn and Winter.	All the Year.
Insanity			
Esquirol's admissions to Charenton { 1,554 cases }	548	452	1000
Parchappe's " " Asylum . { 2,669 cases }	542	458	1000
Penn. Hosp. for Insane, . . { 7,867 cases }	545	455	1000
Crimes against Persons. France { 6,475 cases }	535	465	1000
Suicides : England (1817-1826)	610	390	1000
" Paris (7 years, Dr. Winslow) . .	602	398	1000
" France, 1861-65, men	580	420	1000
" " " women	556	444	1000
" Westminster, England, 1812-1836 .	543	457	1000
" New York City, 1870-1879 . . .	553	447	1000
" San Francisco, 1871-1880	524	476	1000

the season of the year in which they happen, these also will be found most prevalent during the very months when suicide most prevails. In the following table I present such evidence of this phenomenon as I have been able up to this time to obtain.

These facts seem to me too numerous and uniform to be the result of mere accident, but rather as evidence of the theory that suicide and crimes of violence have something in common with insanity. It may be interesting, on this subject, to know that Quetelet, one of the first statisticians of our century, points out in his work, "*Sur l'Homme*" [page 111], that the documents of criminal justice in France make clear the singular fact that the months in which occur the maximum of conceptions, are also the ones in which the most attempts upon chastity are made, and he suggests that this coincidence makes it probable that criminals, in these cases, are carried away by an irresistible impulse. This, however, is a question which does not concern our subject to investigate or discuss at present. The whole question of crime has almost but just begun to attract the attention of students; and there are doubtless mysteries to be here fathomed—of which, to-day, we have little comprehension.

METHODS OF SUICIDE.

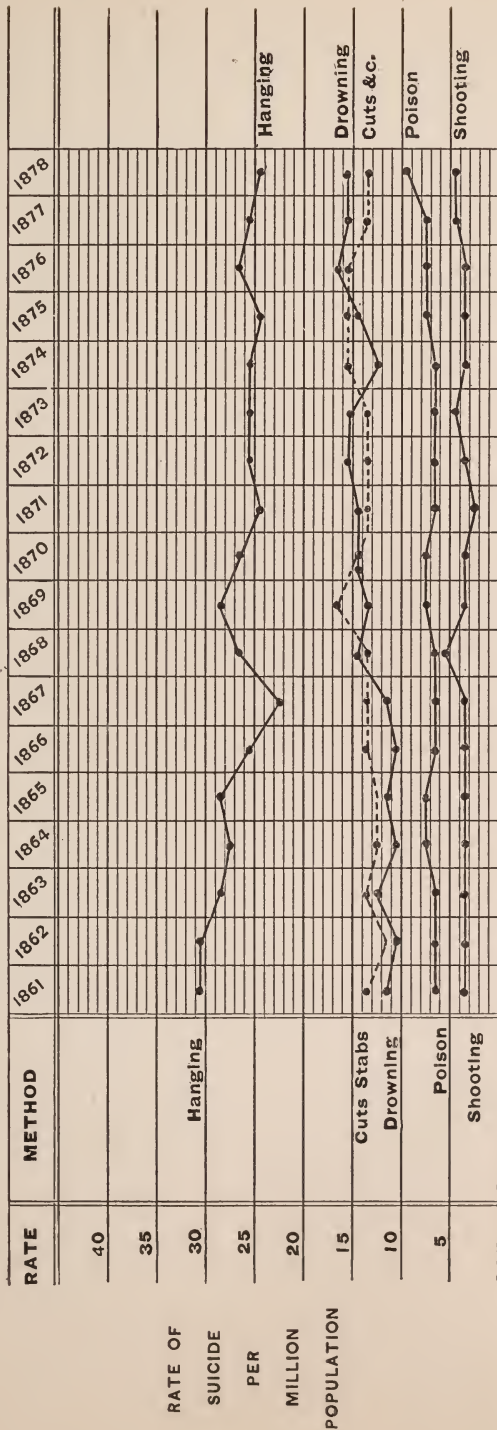
The methods selected by intending suicides offer another suggestive study. The uniformity with which, year after year, a fashion once established persists in claiming followers, has invariably struck statisticians from the first. In the whole realm of nature there is hardly a more wonderful phenomenon than this regularity with which men and women, under a given state of civilization, choose in about the same proportions, in succeeding years, the methods of their death. Take England, for instance. During

eighteen years, 1861-1878, we see by the reports of the Registrar-General that per million population of England and Wales, the number of persons who killed themselves by shooting was exactly three, for thirteen years, varying only during the remainder of the time from two to five; while those choosing poison were either six or seven for every year but the last. I have constructed from the available figures a diagram. See plate, fig. 2.

We see, then, that throughout this long period of many years the fashion of suicide has remained precisely the same, excepting only in a few instances to indicate a preference for water in place of lead.

It is a rather singular circumstance that so many writers on this subject, attempting to speak with authority, have fallen into very palpable errors. If we cannot repose entire confidence in our favorite cyclopædia, where may we turn, or in what authority shall we place our trust? Yet they have invariably erred in attempting to lay down a general and universal fashion to which suicides are supposed to conform. Both "Chambers's Cyclopædia" and the "Encyclopædia Britannica" (8th ed.), two of the highest authorities, name an order of suicide for both sexes in which errors exist. A writer in *Blackwood's Magazine* during the past year ventured to lay down a fashion for self-destructive methods, which is farther than either of the other named authorities from being correct. Some of these mistakes are quite incomprehensible and inexcusable for English writers, to whom the reports of the Registrar-General are accessible. I have taken the trouble to analyze the methods of suicide in England for men and women during ten years, and the figures (which I shall not here occupy space in giving) demonstrate, beyond question, the inaccuracy of these authorities. But it can be proven also by the facts for a number of cities, which are of more inter-

PER 1,000,000 POPULATION, NUMBER OF SUICIDE IN ENGLAND DURING 1861-1878, BY FIVE PRINCIPAL METHODS



Certain deductions from above facts may be stated as follows.

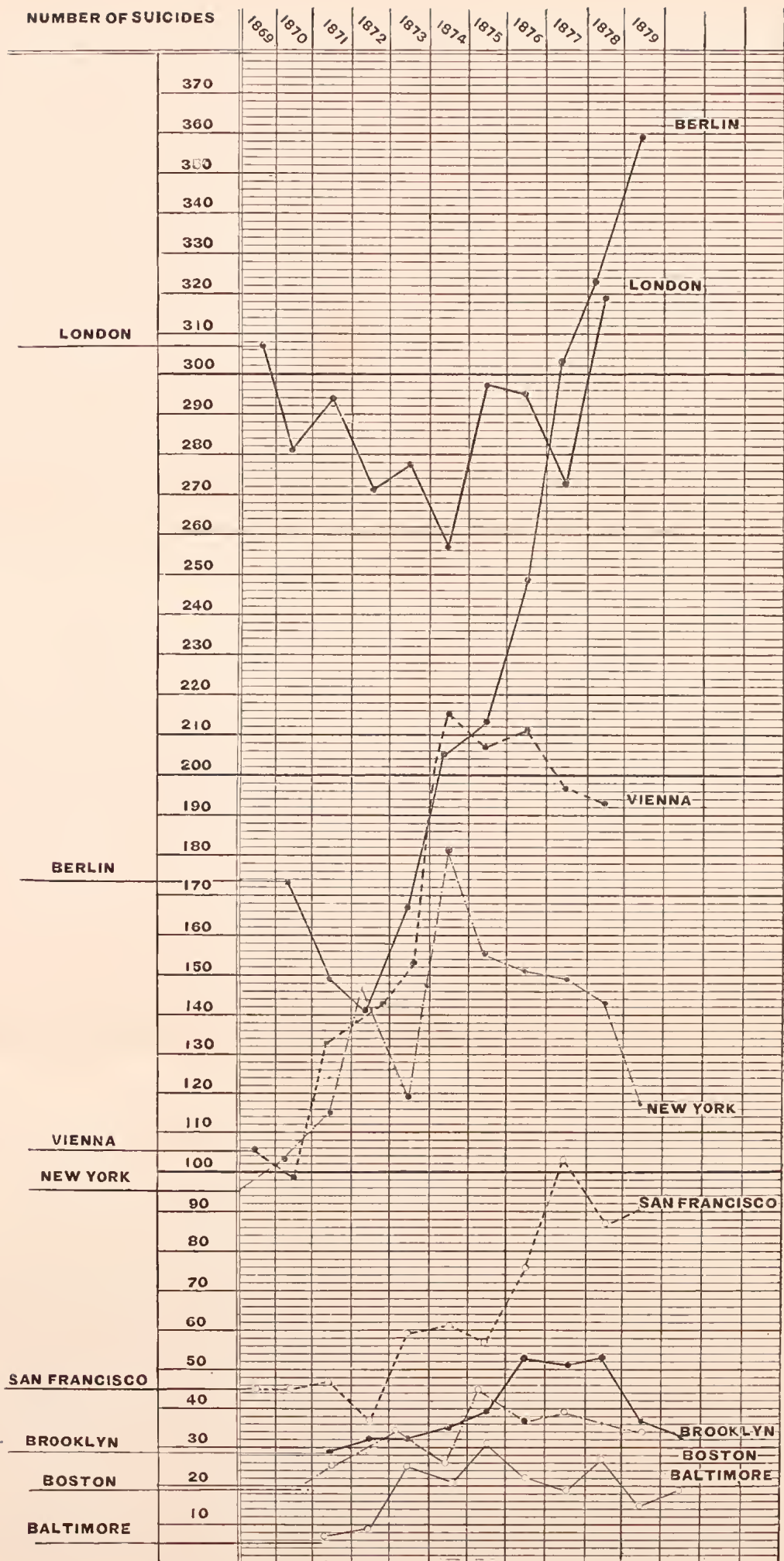
1. It is manifestly impossible for any authority, cyclopædia-writers included, to lay down a fashion in suicide which shall be generally applicable everywhere. Each locality or country preserves its own averages; and the error of writers on this subject has been to imagine an observed order for one country could be stated as applicable to all.

2. Very many suicides, in which design is not easily distinguished from accident, escape record altogether. This is so especially the case with regard to drowning, that it vitiates very materially any accurate comparison between our American cities and those of Europe. I believe it is the rule in New York to count no such death as suicide unless intent is proven, or the act seen; the rest are merely "found drowned." How far from truth this must be we can readily imagine from the experience of a single year. In 1879 no less than 138 dead bodies were taken from the waters surrounding New York, of which 39 were never identified. How many suicides did the coroner ascribe to "drowning" in that year? Seven.

3. The above tables demonstrate that poison is more generally used by suicides in America than in Europe. The reason for this is undoubtedly the greater facility with which it may be procured. In San Francisco, Dr. Dow, the present coroner, states that "no restrictions are put on the sale of poisons, any one at any time being able to obtain from any drug-store all he may desire." What is the result? Between 1875-80, of all the female suicides sixty-five per cent.—nearly two thirds—were by poison! In New York, between 1870-79, fifty-three per cent. of female suicides were by poison.

INFLUENCE OF FINANCIAL DEPRESSION.

The connection between a high rate of suicide and a



general depression in trade and industry is so striking that it deserves remark. Its effect is, of course, remote rather than immediate; it lowers wages, lessens the opportunity of obtaining a livelihood, increases the poverty of those already poor, ruins the small trader, and thus everywhere depresses the tendency to hopefulness and the love of living. In the accompanying diagram (plate, fig. 3) I have shown the rise and fall in actual numbers of suicides in several cities of this country and Europe during a period of ten years. Its value would have been greater could we determine the rate per million population for each year and each city; but this unfortunately is impossible.

A slight rise should be expected from year to year, but the decrease is only explicable on the ground that the previous rise in number was greater than the addition to population. The facts for American cities only can be brought out in a table giving actual numbers of suicides for each year, 1872 to 1879. The greatest number of suicides I have written in black, especially to distinguish it to the eye.

NUMBER OF SUICIDES IN AMERICAN CITIES.

	New York.	Philadelphia.	Boston.	Baltimore.	Brooklyn.	San Francisco.
1872	144	48	22	9	31	37
1873	118	47	26	24	31	59
1874	180	59	16	20	34	60
1875	155	68	39	30	39	56
1876	150	60	27	21	51	75
1877	148	59	34	19	50	103
1878	142	40	31	27	52	86
1879	117	—	28	15	37	90

This table brings into prominence the following points :

1. A wave of suicide passed over this country between 1872 and 1880. The highest level was reached for most Eastern cities in 1874 and 1875. Brooklyn is a marked exception, although the deaths by drowning in 1876 may have been more than in 1878, and so made the suicides of 1876 the majority. In San Francisco the maximum was not reached until 1877, and the depression which prevailed in the east did not touch the Pacific to any great extent until that year. It appears now to be everywhere receding with the general revival in trade and industry throughout the country.

2. In Europe there is a marked contrast between different cities in this respect. In London, the tendency, until 1874, was, on the whole, downward ; but even in 1878, in proportion to population, the rate was lower than in 1869. In Vienna, on the contrary, up to 1874, there was a steady rise in the rate of suicide ; but since then an almost steady decline. Berlin, however, from 1872, has shown an enormous increase in suicides, out of all proportion to population. The people are discontented with their government, unsettled in their faith, and suicide is the simple and legitimate expression of discontent with one world and despair of another.

Here we must end the present study. It would doubtless be of interest to continue the investigation into other fields, to review the various causes which excite or impel the act, to note how far suicide is the result of insane delusion or correct judgment, of selfishness or altruism, of vicious or virtuous lives, of the fear of shame and dishonor, or the pangs of disappointed love. We need also to know to what extent it springs from causes which society might alleviate : from want and destitution in old age, from lack of sympathy in distress, from the discomforts and

pains of disease, which, even when incurable, might be rendered less. It would be valuable to study the effect of religion as a restraining influence, and curious to note whether we might discern any difference in the suasion of diverging creeds. But for due treatment of these topics space is wanting; it must suffice to have imperfectly pointed out a few facts not generally known, and—viewing suicide as a social disease—to have indicated some points in “diagnosis,” leaving for future consideration elsewhere the methods by which, one day, we may hope to restrain impulse, to arrest tendency, and prevent despair.

THE TREATMENT OF WINE-MARK BY ELECTROLYSIS.*

BY GEORGE HENRY FOX, M.D.,

CLINICAL PROFESSOR OF DISEASES OF THE SKIN, COLLEGE OF PHYSICIANS AND SURGEONS,
NEW YORK.

TWO years ago I had the privilege of making some remarks before this Society on three methods of treating the superficial *nævus*, or "wine-mark." These methods were: by linear scarification, as recommended by Mr. Balmanno Squire, of London; by puncture, or so-called "tattooing," as recommended by Dr. Sherwell, of Brooklyn; and by electrolysis. The success which has attended the use of electrolysis in the removal of superfluous hair, first led me to think that the electrolytic needle might be used to advantage in the treatment of wine-mark, by a subcutaneous destruction of the capillary vessels. I did not start with the idea that this plan of treatment would remove a wine-mark and leave the skin in a perfectly normal condition, but it occurred to me that by creating numerous minute cicatrices upon the surface of the patch the color could be so reduced that the mark would present little contrast with the surrounding skin. At the same time there would be no danger of producing a smooth scar of a dead white hue and contractile character, which would be scarcely less disfiguring than the original deformity. At the meeting

* Read before the N. Y. State Medical Society, Feb., 1882.

of the Society referred to, I suggested this operation, and mentioned a case in which I had resorted to it with partial success. To-day, with more experience in the treatment of this affection, I can recommend electrolysis as being, in my judgment, the best method, except for the mildest cases, in which a simpler plan will suffice. The object aimed at in electrolysis, as in scarification and puncture, is to excite sufficient inflammation to destroy the fine network of blood-vessels. As the galvanic current is more active, and, at the same time more manageable than acid adhering to the point of a needle, it is not strange that this method should produce the desired effect in the speediest manner and with the least injury to the surface of the skin.

The operation is quite similar to that which I have already described in a paper read before this Society, on "the permanent removal of hair by electrolysis." A single needle, or an instrument containing a dozen or more needles with points upon the same plane and about two millimetres apart, is attached to the negative cord of a constant-current battery. If these needles are fine and with sharp points they can be quickly pressed into the skin without inflicting much pain, and the electrolytic action which takes place around them as soon as the galvanic circuit is complete, serves to destroy the capillary network



FIG. I.

upon which the existence of the wine-mark depends. This it does with but slight injury to the skin itself. The instrument which I have employed is a small brass disc, with from four to twenty holes, through which are crowded fine cambric needles. This disc (fig. 1) screws on a handle about the size of a short pen-holder. When the needles are introduced into the affected skin and the patient completes the circuit by grasping a moistened sponge electrode attached to the positive cord, a blanching of the tissue for a small space around the needles is immediately observed. With ten or fifteen cells of an ordinary zinc and carbon battery in use the needles should be allowed to remain in the skin from ten to thirty seconds, the exact time depending upon the delicacy of the skin and the effect observed. The blanching of the skin around the needles disappears in a few minutes, and nothing is seen but the punctures. In a day or two a group of dark pits or minute crusts are observed where the needles were introduced. In about three weeks the effect of the electrolysis becomes manifest, although the change may be so slight as to indicate a repetition of the procedure. In a case where a dark patch has existed, and the instrument has been repeatedly employed, the change consists in the transformation of color from purple to dark red, and through varying shades of red and pink to a light orange. At the same time the surface of the patch, upon close inspection, will be found to be covered by minute whitish dots. These are the cicatrices caused by destruction of tissue at the numerous points where a needle was introduced into the skin, and are of so slight a character that they are only apparent upon close inspection.

In every case of wine-mark of a severe grade, characterized by a deep purplish color and a tendency to the formation of angiomata or small venous excrescences, the use of the multiple needle instrument, in the manner which I have

suggested, will certainly produce a most marked improvement in the appearance of the patient in a few months or even weeks. It will not suffice, however, for a complete removal of the mark. Indeed, where there is dilatation of the deeper plexus of blood-vessels, as is frequently the case in an aggravated form of the affection, there may be a slight tendency toward a return of the purplish hue, after this has been removed by the operation. In such cases it is advisable to employ a very fine and flexible steel needle, and to introduce this in an oblique direction beneath the skin to the depth of a centimetre or more. By this plan it is possible to destroy some of the larger vessels through which the blood flows in its passage to and from the superficial capillary network. In this connection it will be remembered by some that Mr. Balmanno Squire, in his first publication on the treatment of wine-mark by linear scarification, advised that the incisions be made perpendicular to the surface of the skin. Later, however, he found that much better results could be obtained by making oblique incisions.



FIG. 2.

In an extensive wine-mark of the face involving the eyelids the single needle (fig. 2) must necessarily be used, and I have been surprised and pleased to note the decided change in the expression of a patient's eyes when the dark

color of the lower lid and malar region has been removed. In a case where the upper lip was involved by the naevus and projected considerably at the angle of the mouth, a diminution of size was effected by piercing the lip with a fine electrolytic needle. But the injection of a few drops of pure carbolic acid beneath the mucous surface of this lip proved to be a more effective and less painful mode of treatment.

In recommending electrolysis in the treatment of wine-mark, it is but just to speak of its objectionable features. In the first place, the operation is a somewhat tedious and painful one, and consequently not adapted to the treatment of children who are not old enough to be annoyed by the disfigurement, and who are therefore unwilling to suffer a little pain for the sake of its removal. I have experimented with local anæsthesia, but have failed to find it of any particular service. Very likely others might have better success with it. In the second place, there is a slight danger of causing suppuration and superficial sloughing of the skin in some cases, and a tendency in other cases to the formation of small outgrowths of a keloidal appearance. In one case I was quite annoyed at an unexpected ulcer, of the size of a split pea, which resulted from my treatment, and left a slight depressed scar upon the cheek; and in two cases trifling ulceration was followed by the development of small, firm, vascular nodules. These were suggestive of keloidal growth at first, but they disappeared in the course of a few months, leaving the skin perfectly smooth. Some who have attempted the removal of wine-mark by Squire's method have reported failure and the development of keloid as the result of their endeavors. Whether these resulting excrescences were really keloidal growths is a question, but the tendency to the formation of vascular nodules, before as well as after operations in

cases of wine-mark, has been noted, and in using electrolysis the production of even minute ulcers should be carefully avoided.

Finally, I would remark that in recommending a new method for the removal of wine-mark, I feel chary of speaking too confidently of its merits, bearing in mind the fact that other methods of treatment, which have been recommended to the profession with more or less ardor during the past ten years, have failed to stand the test of time and trial at other hands. I do not believe it is possible to remove a wine-mark and leave a perfectly normal skin, but I do assert that the most unsightly and disfiguring patches can be greatly improved, if not entirely removed, by the production of numerous punctiform cicatrices, so small as to be scarcely noticeable save upon close inspection. I do not claim that the operation which I have practised accomplishes all that could be desired, but I am sure you will agree that a brilliant and most satisfactory result is accomplished, if we do no more than transform a dark and unsightly stain into a smooth patch of a light pink hue.

As a summary of my experience, I may state briefly that wine-mark is not, as is commonly imagined, beyond the reach of surgical skill; that the most unsightly and disfiguring cases can be greatly improved, if not entirely removed; that in few, if any cases, can the mark be taken away without leaving faint scars, or, at least, a change in the character of the skin; that electrolysis, in my judgment, furnishes the best means of treatment, and that it is especially adapted to those cases in which the mark is more or less venous in character and of a dark purplish color.

THE APPLICATION BY INSUFFLATION OF MEDICATED POWDERS TO THE UPPER AIR-PASSAGES FOR THE RELIEF OF CATARRHAL CONDITIONS.*

By D. H. GOODWILLIE, M.D.,

NEW YORK CITY.

TO the general practitioner the local treatment of catarrhal conditions of the upper air-passages too often becomes an irksome duty.

The reason no doubt lies in the fact that when such conditions excite attention there is no efficient means of local treatment. Chronic naso-pharyngeal catarrh, in the majority of cases, has its beginning in early life and comes under the observation of the general practitioner.

A rhinitis, neglected in childhood, with the accompanying hyperplasia, resulting in hypertrophies of the tissues, and followed by the consequent malformations of various kinds, preventing normal respiration and creating naso-pharyngeal trouble in adult life. Hence the importance of early treatment, and so claims the attention of the family physician.

He may not be able to treat all cases, particularly those of a chronic character, as well as an expert, but there is no doubt that with the proper attention early given, with some efficient means, both local and general, he will not only give much relief, but prevent trouble in adult life.

* Read before the State Medical Society at Albany, Feb. 9, 1882,

But I cannot trespass on your time to give here the etiology of naso-pharyngeal catarrh, however interesting it may be, or to speak of general treatment.

My object at this time is to engage your attention to an efficient means of local treatment for general use.

I claim no originality in it, only to make the means a little more efficient to bring relief through your ministrations.

This local treatment consists in making application of finely triturated medicated powders by means of an improved insufflator.

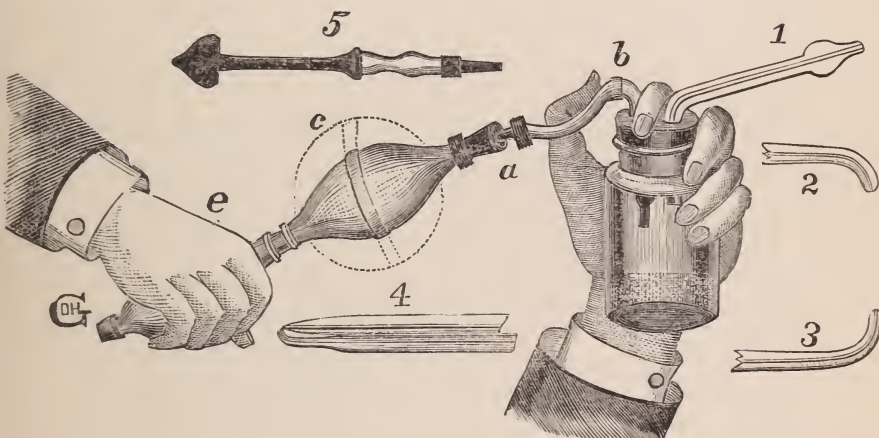


FIG. 1.

The insufflator, fig. 1, is in principle the same as the one brought to the attention of the profession some time since by Dr. A. H. Smith, of New York. Glass tubes being now substituted instead of hard rubber, with valves inside the bottle, so that it is closed when not in use.

It consists of a bottle—that can readily be held in the hand—which contains the medicated powder. Through the cork pass two thick glass tubes with valves inside the bottle, one of which (1) conveys the powder from the bottle and has an enlargement on its end to fit the vestibule of the

nostril. Two other tubes are also made use of to make application to the larynx (2) or posterior nares (3). The other tube (*b*) passes also through the cork and has a valve on the end within the bottle. The other end is attached by rubber tubing to the rubber balls that supply the air. The rubber balls are readily joined and disjoined to the bottle by a hard rubber attachment (*a*).

The air is supplied by two rubber balls (*e*, *c*) closely joined together with a valve between them. One ball (*e*) is worked in the hand, and forces the air into the other ball (*c*), which can be distended according to the amount of air-force required.

The calibre of the bottle and the glass tubes are suited to the capacity of the air-balls.

When an application is to be made, take the bottle in the hand, and with the thumb press firm on the rubber tube over the end of the glass air-tube (*b*), so as to close entirely the passage of any air into the bottle. Now force by the hand the air in the rubber ball (*e*) into the ball (*c*), and distend it according to the amount of air-force required. Before an application is made, the nasal cavity should be as thoroughly cleansed as possible, then the powder blown in. The first application will cause an increased secretion from the muciparous glands, and possibly excite a sneeze. Then the nose should be blown, after which the powder should again be effectually applied.

To make an effectual application to the nose, pharynx, and larynx, it may be done in the following manner.

Distend the air-ball (*c*) of the insufflator, then direct your patient to shut the mouth and exhale through the nose; the moment he ceases to exhale apply the nozzle of the tube into the nostril, then direct the patient to inhale; at the same instant make the insufflation, and the whole surface from the anterior nares to the larynx has an application of the powder.

If there is any stenosis in the nostrils, this cannot be so well accomplished, and it will become necessary to make the application post-nasal also, by using the proper tube (3).

When an application is to be made to the larynx only use the down tube (2).

In the same manner spray can be used if desired, but more force will be necessary (4).

The tube 5, fig. 1, is for inflating the deeper ear passages, and can also be used to blow mucus from the posterior nares when there is stenosis in the anterior nares.

IMPALPABLE MEDICATED POWDERS.

The success of local treatment lies in a great measure in the properly prepared therapeutic agents and their effectual application. An impalpable medicated powder will be found of great value for general use, more especially for the nasal cavity.

But very much of its therapeutic value lies in its proper trituration. Extremely minute division is absolutely necessary to obtain good results.

When proper trituration is attained the impalpable powder when blown into the air floats like smoke, or when blown into the air-passages it passes into every part.

The valuable gums employed in some of the powders cannot be effectually trituated in small quantities and retain their therapeutic value.

I have found that to do this properly it is much better to triturate it slowly in large quantities and at a cold temperature. This prevents frictional heat, which has a tendency to impair the therapeutic value of the gums especially.

The following powders have been found most useful for general use.

	NO. I.	gm.
℞	Benzoini,	3 i—4.
	Morphine muriat.,	gr. vi—0.35
	Bismuthi sub-nitrat.,	} āā 3 ss—15.
	Potassii nitrat.,	

Valuable for its sedative action. To be used in hyperæmic conditions with pain. In the beginning of an attack of rhinitis coat the mucus surface with it.

	NO. II.	gm.
℞	Aluminis,	3 i—4.
	Acaciae,	} āā 3 iv—15.
	Bismuthi sub-nitrat.,	
	Potassii nitrat.,	

Useful where a strong astringent is indicated.

In case of hemorrhage from the nose, remove all the clot and immediately blow in this powder abundantly until the bleeding ceases.

	NO. III.	gm.
℞	Iodoformi, }	āā 3 i—4.
	Camphoræ, }	
	Bismuthi sub-nitrat., }	āā 3 iss—48.
	Potassii nitrat., }	

A good antiseptic.

To be used where the discharges are fetid, or where ulceration is present, or an excessive amount of granulations.

The camphor masks the odor of the iodoform.

These powders when impalpable and with the therapeutic integrity of the drugs preserved can be more effectually applied to the nasal passages than spray, and their good effect is certainly more prolonged.

For the general practitioner they are vastly more convenient than sprays.

The insufflators are made by P. H. Schmidt, of Broadway and 34th Street, New York, where also these powders can be obtained by the ounce. A set of three insufflators with nasal speculua and a tongue spatula can be had in a convenient portable case.

THERAPEUTIC CONTRIBUTIONS.

IV.

ON THE EFFICIENT DOSAGE OF CERTAIN REMEDIES USED IN THE
TREATMENT OF NERVOUS DISEASES.*

By E. C. SEGUIN, M.D.

MR. PRESIDENT AND GENTLEMEN: I have been led to prepare this paper by the following consideration. I frequently see uncured cases of nervous disease for which the attending physician has prescribed the proper remedy, but having exhibited it in doses which, though justified by medical authorities, were wholly insufficient to influence the disease, he has failed. This has been more especially true of chorea, of cerebral and spinal syphilis, of certain neuralgias. In these cases the physician had been wanting in the experience and in the courage necessary to fight his way through opposing tradition and book-authority to success.

There are several evident causes for this timidity, which in a negative way is nearly as injurious to the patient as too great rashness would be.

In the first place, the influence of teachers in medical schools and of writers of text-books is thrown in favor of small or medium doses. Few if any teachers or writers take special pains to indicate the maximal doses of potent drugs;

* Read before the Medical Society of the State of New York, Feb. 7, 1882.

they teach in a condensed form, and with an eye to the safe training of students. This is very well as applied to students, but a time comes when a physician in active practice wants to know just how much physiological effect he can obtain with certain remedies without positively endangering his patients' lives. In the present state of our medical literature, unless he have time and opportunity to hunt through the files of the leading medical journals for detailed observations, or to read monographs on experimental therapeutics, he must work out his maximal doses for himself at the cost of much time, of some anxiety, and of not a few failures.

It seems to me that works on therapeutics intended for the practitioner should give, for each important, physiologically active remedy, a paragraph on maximal doses, clearly indicating the amounts necessary to produce the physiological effects (on man), which are often inseparable from remedial effect. These data should be taken from monographs and special articles on the subjects by men who have had experience in the use of the drugs mentioned. For I take it as granted that it is now just as impossible for one man to give us a satisfactory, practical work on therapeutics, as it is for one to produce a uniformly excellent work on the practice of medicine.

In the second place, I have observed that many capable druggists are alarmed at doses of certain remedies which are not only harmless, but essential to success. I clearly remember that, when a student, I heard the late Prof. Freeman J. Bumstead relate, with a mixture of amusement and anger, how a leading druggist had sent to him to inquire if he really meant to give 3iss of bromide of potassium at one dose. This was twenty years ago. Yet, only a few days since, a patient told me that her druggist told her that she must have a very strong stomach to stand such powerful medicine (she was taking gm. .004 of biniodide of mercury

and gm. 3. of iodide of potassium three times a day, and under this in one week had lost nearly all her syphilitic pain). Very frequently have I had prescriptions for my usual doses of Squibb's conium returned for revision by the careful pharmacist. I intend nothing derogatory by these remarks, for druggists are supposed to know only the maximal doses of remedies as given by books, and they but do their duty in sending a prescription back for revision, if any thing in it seems wrong. For my part, acknowledging a liability to error, I am always glad to see this healthy doubt applied occasionally to my prescriptions; yet I would not have physicians allow themselves to be influenced by the remarks or practice of druggists. Philosophically the two professions are absolutely separated: the one furnishes the other with the proper implements of treatment in the best possible condition; and it is the function of the physician to determine by scientific knowledge and by experience how, when, and how much these implements shall be employed. In more senses than one the physician is responsible for the dosage of remedies.

In the third place, it has seemed to me that our large manufacturing drug-firms exert a baneful influence upon therapeutics. They have flooded the country with formulas and ready-made compounds, and thus relieved the physician of the necessity of exerting his power to extemporaneously devise the compound required for the individual patient before him. Increasing numbers of physicians, instead of adapting the materia medica to their patients, practically adapt their patients to an already-prepared stock of elixirs, pills, and mixtures. It is so convenient to order one of these, so much easier than to weigh the indications presented by the case, to estimate the patient's susceptibility, and then to write out a good prescription for the case, or more exactly speaking, for the patient.

I propose to briefly review the posology of a few drugs—giving the doses as stated by the best authorities, by writers on therapeutics, and by clinicians, and then stating the doses which I believe to be useful and safe.

I wish it particularly understood that in advocating larger doses of these remedies I do so only on the basis of a tolerably large experience, and not at all from any theoretical scientific considerations. At the same time that I advocate efficient doses, I am carefully observant of all the circumstances which render patients susceptible, and always make an allowance for idiosyncrasy. Thus, in first prescribing a potent remedy, I take into consideration the age, sex, and size of the patient; and also make an estimate of his general condition, and note particularly the state of his circulatory organs. Then, for a patient whom I see for the first time, I order very small doses, doses such as the books justify, and by steady increase feel my way, fearlessly because watchfully, to the larger doses, often seemingly dangerous doses, which really affect the organism and may cure the disease.

In this matter I make no claim to originality, and would not affirm that the doses I recommend are always essential to success; I simply sum up my experience and place my results at your service.

I.—FLUID EXTRACT OF CONIUM.

(*Extractum conii fructûs fluidum*.—U. S. P.)

Doses as given by authorities on therapeutics and materia medica:

Is not mentioned by STILLÉ and MAISCH, by STILLÉ, by NOTHNAGEL, and by GUBLER.

WOOD. Therapeutics (1880), p. 371. Dose, ℥ 1 to 2. (!)

BARTHOLOW. Materia Medica (1880), p. 409. Dose, ℥ 2, ℥ 5, increased to ℥ 40.

RICE. Posological Tables (1879), p. 28. Dose, from 3 to 5 minims, to be increased with caution.

Doses as given by clinicians :

Conium, in the form of fluid extract, is not, to my knowledge, mentioned by any standard writer on the practice of medicine.

MEIGS and PEPPER, Diseases of Children (1870), p. 505, article chorea, refer to Dr. J. Harley's doses of succus conii with apparent astonishment.

To Dr. JOHN HARLEY (The Old Vegetable Neurotics, London, 1867) we owe the present rational or physiological use of conium. He swept away the former traditions of the potency of the drug, and showed that most of its preparations were inert. He obtained definite physiological and therapeutical results from the succus conii, administered in doses of from 3 ii (gm. 8) to ʒ i (gm. 32). By means of these quantities he obtained the paresis of third nerves, arms and legs, which is the characteristic result of conium action on the spinal cord.

The prototype of our excellent officinal preparation, the fluid extract made by Dr. Squibb, was unknown to Dr. Harley until just as his book was going to the press (p. 94, note).

Dr. Squibb and Dr. Manlius Smith had, however, read a paper before this Society, at its meeting in 1867, entitled : "An attempt to answer the question, Which part of conium is the best for medicinal use?" (See transactions of the New York State Medical Society for 1867.)

Ever since, we, on this side of the Atlantic, have possessed by far the most reliable and the most powerful preparation of conium; but I am sorry to add that it has been used rather inefficiently, and that even intelligent physicians are afraid to use the only doses which have any effect.

I have used conium a good deal in the last ten years, and have always employed the fluid extract as made by Squibb. I have tried it in chorea, in spasm of paralyzed limbs, in general irritability, and in insomnia.

When the indication is present, as in chorea, to obtain muscular relaxation, after a few tentative doses of 20 and 40 minims, I give at one dose 60, 80, or even 100 minims. These doses cause drooping of the upper lids (sometimes diplopia) and paresis of the arms and legs. I do not repeat the dose until after all the effects have passed off—in from 12 to 24 hours.

In a case of chronic adult chorea of 14 years' standing, which I almost perfectly cured in 1872-3, at the Epileptic and Paralytic Hospital on Blackwell's Island, a large part of the result (a very remarkable result in my experience) was attributable to paresis daily produced by a teaspoonful of Squibb's extract of conium for a month or more.

Many cases of insomnia, with wakefulness in the first part of the night, more especially those with fidgets or physical restlessness, are very much benefited by conium. I usually give 20 minims, with 20 grains of bromide of sodium in camphor water, at bedtime, to be repeated if necessary. In some cases (male adults) I give 50 or 60 minims at one dose in the mixture, not to be repeated. Such a sleeping-draught prescription has been repeatedly returned to me by druggists, because they thought the dose enormous. Indeed, I usually warn patients that the druggist may comment on the dose.

If we have a clear indication to give conium, we ought to give enough to fulfil the indications, and this cannot be done without obtaining the physiological effects. With due precaution, there is a wide and sure distance between physiological and toxic effects, yet, with reference to remedies

such as I shall refer to, how few physicians understand and appreciate that the curative effects are obtained in just that interval between physiological and toxic effects. To be successful we must be bold, as bold as physiological knowledge can make us, and yet as cautious in the first giving of powerful drugs to a patient as if we had no courage at all.

(To be continued.)

EDITORIAL DEPARTMENT.

A SECOND YEAR'S EXPERIENCE WITH NON-RESTRAINT IN THE TREATMENT OF THE INSANE.

In 1794 Pinel removed the chains from the insane patients in the Bicêtre. At about the same time, or a little later, the Retreat at York was built and placed under the superintendence of William Tuke. Here the patients were treated with very little restraint ; but neither of these went so far as to entirely abolish restraint apparatus ; it was reserved for Charlesworth to attempt and for Gardiner Hill to carry it out at Lincoln, to be followed by Conolly at Hanwell, who confirmed its practicability and advantages on a large scale. Hanwell had at this time 800 patients. It is of interest to follow the various arguments which were then used by superintendents and others against the non-restraint system, and the numerous misrepresentations which were made as to the use of non-restraint by Dr. Conolly ; but in spite of these unjust criticisms, which were either intentional or due to that peculiarity of the human mind which causes it to oppose whatever is new, one by one the English superintendents adopted it, and to-day in Great Britain it is the almost universal practice.

The superintendents of American asylums have almost unanimously decided to defend the use of restraint apparatus, and to-day they use the same arguments and derisive sneers against non-restraint as were used in England in the days of Conolly. They fail to see that these arguments availed nothing in opposing the spread of the non-restraint system in Great Britain.

These superintendents say of an asylum or asylums which carry out non-restraint, "That they do not do it ; it is a myth " ; and feel perfectly satisfied that they have demolished the whole practice of non-restraint and its advocates.

I cannot avoid quoting from Conolly, for his remarks are so pertinent and applicable, and give exactly the arguments used to-day by superintendents. He says : " One general error seems also to pervade the minds of those who most severely condemn the abolition of restraint ; they always assume that if one kind of violence is discontinued, some other kind of violence is substituted for it."

They also affirmed that the patients were locked up in cells, etc. It was at this time also that originated among these opponents of Conolly the appellation of "chemical restraint," and which they claimed was the substitute for mechanical restraint. To-day, as then, the "chemical-restraint argument " is being constantly used. If non-restraint is and can be carried out in one or more asylums, that fact is sufficient answer to all objections of quibblers. And, if it is carried out, who could possibly question which is the better, the asylum with non-restraint, or the one with restraint ?

It is now two years since the adoption of non-restraint in the Kings County Insane Asylum, and from the experience during that length of time, the following has been found to be the difficulties to be surmounted and the advantages to be gained by carrying it out. The difficulties in the commencement are the prejudices on the part of attendants and others, against the abolition of restraint. Attendants who have been brought up in asylums where restraint has been carried out, are usually unable to understand how patients can be managed without, and they will even oppose it, and tell the medical officer that it is impossible ; and if he still persists in his efforts to get the patient out of restraint, they will fraudulently try to convince and show him that it is impossible. They will even in certain cases go so far as to aid and urge the patient to do violence to other patients, or irritate them so as to cause great excitement, and thus carry their

point, and show the medical officer that he is wrong and they right. I have known this to be done, and it is sometimes not difficult to convince medical officers in this way. An attendant who shows the least desire to frustrate any plans of a medical officer should be immediately dismissed ; and nothing short of that will do at the outstart, for one bad attendant will spoil many.

Having restraint apparatus in an asylum has to a certain extent a demoralizing influence upon medical officers as well as attendants. Whatever difficulties may arise, there at once appear before them visions of camisoles, muffs, straps, etc., hanging on pegs ; and it is so very easy to solve the difficulty and save an immense amount of trouble and mental perplexity to the medical officers, that they order the attendant to put on the camisole. This also suits the attendant, for he, to be sure, has a struggle with his patient, but he gets him in the camisole, and then very often thrusts him into a room and is rid of him.

If there is no restraint apparatus the medical officer is obliged to go to his patient and find out what has been the cause of his excitement, and learn all the surrounding circumstances, and from this to find a way of managing the patient.

One of the great difficulties, especially with old attendants who have been brought up in asylums, is, to prevent and make them understand not to interfere with patients unnecessarily ; for instance, take a comparatively quiet ward, as you go through you will find the attendant making the patients all sit down in rows against the wall. And I have even seen one superintendent walk through his ward when all his male patients were sitting in this way ; one of them quietly arose from his seat, when he was at once pushed by the medical officer himself into it again.

This disposition to interfere unnecessarily with patients can be seen in its mildest form by any one as they pass through an asylum, even when accompanied by a medical officer. The patients arise and walk toward you ; observe now the attendant trying to make them go away and sit down ; this is very hard to make attendants believe they must not do. Two motives appear to actuate them in this : 1st, out of deference to the medical officer

who, they think, does not wish them to be disturbed ; or, 2d., because of the evident fear which some people display in passing through the wards of an asylum, and which is due to the common idea that an insane person is like a wild beast.

Take another example, an excited man with chronic or acute insanity. He is in a ward walking up and down excitedly, perhaps talking loudly: the attendant follows him about: he goes into a bedroom ; the attendant pulls him out : he goes to the hall door, perhaps tries the knob, or kicks the door ; attendant rushes after him and pulls him away : he goes to the window ; attendant thinks he is going to try and break out, takes him away from there and tries to make him sit down : each effort of this attendant has been making the patient more and more angry, and at last he strikes the attendant ; he is put in a camisole after a great deal of struggling, and is hereafter looked upon as a bad man. Now all this might have been obviated and the camisole unnecessary if the attendant had only let this man alone ; patients should only be interfered with when they are doing injury to themselves or others, or destroying furniture, etc.

This idea that patients must be followed about and put in restraint apparatus if they manifest the least excitement of manner, etc., so dominates the minds of attendants and others in charge of asylums that it is hard for them to believe that patients can be managed otherwise.

This is shown by a recent experience. The clerk to the Kings County Asylum, Mr. Edward Shannon, recently took eight patients to the asylum at Binghamton. Several of them were cases of chronic mania. One of them was particularly talkative and demonstrative, constantly moving about, talking in loud voice, gesticulating, etc. This man had never been in restraint (he had been in the asylum for a year and a half). These eight men were taken to Binghamton without any restraint apparatus by Mr. Shannon, in company with Dr. Hoyt (the patients were State paupers). On the way, patients from other asylums came on the train, and these patients all wore restraint apparatus, although they were cases of senile dementia and quiet terminal dementia. On ques-

tioning the persons who accompanied these patients, Mr. Shannon was told *that they were quiet patients, but they thought it was best to put them in restraint.*

Here was a marked contrast—one group of patients noisy, talkative, demonstrative (but harmless), without restraint apparatus; the other group quiet, feeble, demented, inoffensive, all in restraint.

With ordinary suicidal patients they can be cared for easily without restraint of camisole or crib, and prevented from committing suicide by a faithful and competent night-watch having these patients under observation all night. They are generally quiet, and if in sight will not try to injure themselves; but it sometimes happens that you meet with a patient who makes continued efforts of all kinds to commit suicide. In the two years that non-restraint has been carried out at the Kings County Asylum one such patient has been met with. She made many suicidal attempts. She would try to suffocate herself in a straw bed; she would cover her head with her blanket in the presence of nurse, and try to strangle herself with her hands; she would strike her body on the floor, strike her head against any thing that was hard—wall, floor, bedstead, etc.; tried to pull her tongue out and bite it off. She had advanced phthisis, extensive laceration of cervix, and atresia vaginæ of recent origin, the result of sloughing of vagina at a very recent labor. These are very difficult cases to manage, as it is almost impossible to prevent them from bruising themselves, no matter what means you employ. The camisole would not prevent such a patient from striking her head against the wall, etc.; the crib would be as useless, for they could strike themselves against it. The padded room, with watching of attendant, appears to be the best method of caring for such a patient. Fortunately these very trying cases are not frequently met with.

Seclusion of patients (that is, removing a patient to a room and locking the door so as to retain him there against his wish) is a method which can very easily be abused, unless special care is exercised; and it is true that it may be made to take the

place of another abuse, namely, restraint apparatus. The seclusion of patients must be kept under the control of the assistant medical officers, supervised by the medical superintendent, and this seclusion should be discontinued just as soon as it is possible to do so. Attempts should be made from time to time to keep the patient out in the hall. In this way you are less likely to keep the patient in seclusion longer than is necessary. In an asylum which is not over-crowded it is much easier to get along without seclusion, or with a very small amount, than in one very much crowded ; for, as a rule, seclusion has to be resorted to for preventing a patient from injuring others.

It is hard to conceive how any one can prefer the use of the camisole to seclusion. If one who has seen them both used asks himself which of these two methods he would rather have applied to himself, I think he would be very apt to prefer a temporary locking up in his own room. Besides, a man who is violent and excited, with a camisole on, can walk up and down the hall among his fellow-patients, and, if disposed, can do them just as much damage as if his hands were free, because he can and will kick them just as much as he pleases. In a room he cannot do this.

It is sometimes said by persons visiting an asylum without restraint apparatus that the patients in this asylum are not the same kind which are found in another asylum where restraint apparatus is used. This is a very great error, and if any one has seen for some length of time both methods of management, he will become convinced of it.

The truth is that the patients do really differ as far as their conduct is concerned, but no farther. They are probably all of the same nationalities and come from the same walks in life, but in the asylum without restraint the patients are quieter and better behaved, simply because they have not been and are not irritated by restraint apparatus. And a patient first admitted to an asylum and put in restraint can soon become a violent patient from this cause alone.

And it is in this way that the asylum with restraint, from its method of management, forms the violent character of its patients;

and it is in this alone that the patients in the two asylums differ. One irritates the patients with restraint, and the other does not.

This is very clearly shown by Dr. McDowall in a review of an article by Dr. Billod, published in the *Journal of Mental Science*, Jan., 1882, page 596. He says Dr. Billod is right when he says that the use of restraint is simply a matter of asylum organization. "Of this I had a crucial example during my visit to Denmark three years ago. On the island of Zeeland there are two asylums, and they are managed on entirely different principles. Both are conducted by thoroughly competent men, who devote themselves heart and soul to their work, and who carry out their ideas of right with great consistency. Yet the contrast between the establishments is surprising. At Roskilde Dr. Steinberg carries out the practice of non-restraint, and as one walks through the wards and grounds one is at once reminded of asylums as they are at home.

"Seclusion is rarely used, and restraint almost never. This asylum receives most of its patients from Copenhagen. The cases are largely composed of general paralytics and acute maniacs, due to alcohol.

"An entirely different system of management prevails at Vortenburg, a large asylum for the rest of the island. The management is perfect in its way, but it is thoroughly French. If on admission a patient is excited and destructive, he is secluded and restrained until the excitement disappears.

"I therefore saw a patient in every single room in the asylum, and some had been in these rooms from a few weeks to more than twenty years. It must be stated that the single rooms are much better than those in English asylums,—large airy rooms with windows in the roof. Across these windows it is possible to draw blinds to limit the amount of light admitted. Dr. Fürste found that the amount of light influenced the excitement; the more light the more excitement. There were strait waistcoats in abundance. It must not be imagined that all this restraint and seclusion are employed to save trouble. They are employed on principle as the best method of treatment.

"The staff is large—one attendant to six patients—and the supervision by the superior officers thorough and untiring. Dr. Fürste spends far more time in the wards than any English superintendent I know—on an average ten hours a day; he is evidently popular with his patients, many of whom told me that they are very kindly and considerately treated. In spite of all this I could not help condemning a system which locks up patients for years, and I can never forget the case of an old woman, an amusing chronic maniac, who had not seen the sun or a blade of grass for more than twenty years.

"My Danish experience settled the matter to my mind. Without denying the influence of racial and other difficulties, it is certain that there is no absolute obstacle to the adoption of the non-restraint system."

It has also been claimed now, as in the days of Conolly, that with the abolition of restraint apparatus there was instituted to take its place the use of sedative drugs, and which was styled "chemical restraint." Conolly denied this, and to-day we have the best argument against this fallacy, in the tables in Dr. Wilbur's article showing the relative amount of sedatives used in the asylums with restraint and those without. Dr. Wilbur's tables are here reproduced (see ARCHIVES, vol. vi, p. 271).

TABLE NO. I.—BRITISH ASYLUMS.

NAME OF ASYLUM.	No.	Number of patients	Monthly occasions of restraint.	Monthly number restrained.	Monthly occasions of seclusion.	Monthly number secluded.	Average number to whom chloral is daily administered.	Average number to whom hyoscyamia or other narcotic is administered to allay excitement.
West Riding Asylum . .	1	1,410	1	1	2	2	31	38
County Asylum, Chester .	2	533	None	None	None	None	None	Morphia used occasionally
Hull Borough Asylum . .	3	163	"	"	"	"	1	2
Montrose Roy'l Lun. Asl.	4	485	"	"	19	"	3	1
Brookwood Asylum, Surrey	5	1,050	"	"	None	None	10	1
East Riding Asylum . .	6	285	"	"	"	"	$\frac{1}{2}$	None
Hanwell Asylum	7	750	"	"	"	"	None	"
Burntwood Asl., Litchfield	8	600	"	"	"	"	"	"
Royal Edinburgh Asylum .	9	832	"	"	20	"	1	"
North Riding Asylum . .	10	546	2	1	3	"	7	9
Royal Asylum, Gartnavel .	11	483	None	None	None	None	6	14
Richmond Dist. Asl., Dublin	12	1,013	"	"	3	1	11	2
Dr. Hill's Norfolk Co. Asl. .	13	620	"	"	None	None	None	20
Kent Co. Asylum	14	1,200	"	"	"	"	"	None
Woodilee, near Glasgow . .	15	448	"	"	"	"	"	"
—		10,419	3	2	47	4	70 $\frac{1}{2}$	91

TABLE NO. 2.—CANADIAN ASYLUMS.

NAME OF ASYLUM.	No.	Number of patients	Monthly occasions of restraint.	Number restrained.	Monthly occasions of seclusion.	Number secluded.	Average number to whom chloral is daily administ'ed.	Average number to whom hyoscyamine or other narcotic is administered to allay excitement.
Nova Scotia Hos. for Insane	1	380	117	11	17	5	None	None
Lond. Ont., Asl. for Insane	2	851	61	8	13	9	"	"
Toronto Ont. Asl. for Insane	3	673	10	3	4	2	2	3
Hamilton Ont. Asl. for Ins.	4	537	6	—	4	—	5	5
Kingston Ont. Asl. for Ins.	5	430	4	—	13	—	occasional	occasional
		2,871	198	—	51			

TABLE NO. 3.—ASYLUMS IN UNITED STATES.

NAME OF ASYLUM.	No.	Number of patients	Monthly occasions of restraint.	Number restrained.	Monthly occasions of seclusion.	Number secluded.	Average number to whom chloral is administered daily.	Average number to whom hyoscyamine or other narcotic is administered to allay excitement.
Northern Hospital, Wis.	1	541	48	—	1	1	24	8 daily
Cook County Asylum, Ill.	2	440	480	—	60	—	33	—
Kings County Asy'm, N. Y.	3	868	None	None	—	8	52	6 a month
Wooster Hospital, Mass.	4	594	69	—	71	—	22	No record
Retreat for Insane, Hartford, Ct.	5	121	2	—	2	—	4	1 daily
Willard Asylum, N. Y.	6	1,727	—	6 daily	7	—	27	10 daily
Athens Asylum, Ohio	7	586	None	None	116	58	20	None
Longview Asylum, Ohio	8	661	8	—	11	—	8	1 daily
Dayton Asylum, Ohio	9	591	118	6 daily	309	—	29	—
Northern Asylum, Elgin, Ill.	10	526	483	—	25	—	26	5 daily
Insane Criminal Asy'm, N. Y.	11	131	1	1	—	—	14	5 a month
Middletown, Conn.	12	582	16	3	43	15	21	1 daily
Minn. Hospital for Insane	13	530	67	43	24	17	6	2
Southern Asylum, Anna, Ill.	14	486	350	—	129	—	12	3
Eastern Illinois Asylum	15	175	—	1	22	—	—	3
Homoeopathic, N. Y.	16	244	5	3 daily	—	—	None	None
Central Hospital, Illinois	17	641	483	32	2	—	73	48
Western Asylum, Kentucky	18	473	16	—	12	—	9	8
Hudson River Hospital	19	250	—	6	None	—	29	12
State Insane Hospital, Wis.	20	548	2,547	—	13	—	4	10
Danvers, Mass.	21	643	138	—	161	—	5	3
Northampton, Mass.	22	471	292	25	248	26	None	None
State Lunatic Hospital, Harrisburg, Pa.	23	353	3	—	21	—	18	3
Taunton Lunatic Hospital	24	574	14	—	6	—	20	—
N. J. S. Lunatic Asylum	25	581	180	—	150	—	118	None
Newburg, Ohio	26	625	24	—	38	—	47	21
		13,967	—	—	1,254	—	561	139

This table is incomplete in some of its columns because the reports upon which it is based were imperfect.

I have reason for believing that since these tables were drawn up by Dr. Wilbur, an improvement has taken place in these asylums.

J. C. S.

Asylums with restraint use as much of sedatives if not more than those having no mechanical restraint. If this were a correct argument, the asylums without restraint should use a great deal more than those with restraint, which they do not.

The use of the words "chemical restraint" for the administration of sedatives to excited patients is incorrect. Within the proper limits which govern the giving of sedatives, and in fact all medicines, it is justifiable, even proper and necessary, to give sedatives to an excited insane person, as to give opium to a person in physical pain or suffering from peritonitis.

Any one who says that it is unjustifiable to give a man sedatives who is suffering from terrifying hallucinations from excessive use of alcohol, for instance (even if they be of temporary effect), while in connection you give him large quantities of food, etc., for the purpose of restoring him to his normal condition, simply talks nonsense. And it must never be forgotten that a great deal of useless argument and criticism are indulged in by the profession on this subject of giving drugs.

The experience at the Kings County Asylum has been, that with the abolition of restraint, there has been a gradual diminution in the amount of sedatives given both by day and night, and to-day it is very small.

It has always been thought that to carry out non-restraint a large number of attendants were required; this has also been proved to be incorrect by the experience of the Kings County Asylum, where there is one attendant to fifteen patients on the average. And from recent reports of the Commissioners of Lunacy in England and Scotland, it appears that in their asylums there is on the average not more than one attendant to twelve patients.

But one of the most important points in carrying out the system of non-restraint is to find occupation for the patients. This aids very much in keeping them quiet and more contented, tends to turn their attention to a more normal train of thought, and in some cases prevents the rapid approach of complete dementia.

One great difficulty with American asylums, and it appears to be the same to some extent in England, is the want of occupation for men in winter, and the difficulty of getting both sexes out of doors in winter. The want of out-door exercise and recreation causes a restlessness, and makes it much more difficult to get along with them. It is hoped that a solution of this will be found ere long.

It must not be inferred that restraint apparatus is used to great excess in all the asylums in this country. There are certainly a great many where it is carried to its fullest extent (see article by Dr. Woodside in *N. Y. Medical Record*, March 4, 1882) ; but it is gratifying to learn from gentlemen well acquainted with many asylums that there has been a diminution of the use of restraint apparatus within recent times, and doubtless this will continue following the same course as it did in Great Britain.

NEW BOOKS AND INSTRUMENTS.

A New Freezing Microtome. By Dr. WILLIAM HAILES, of Albany, N. Y.

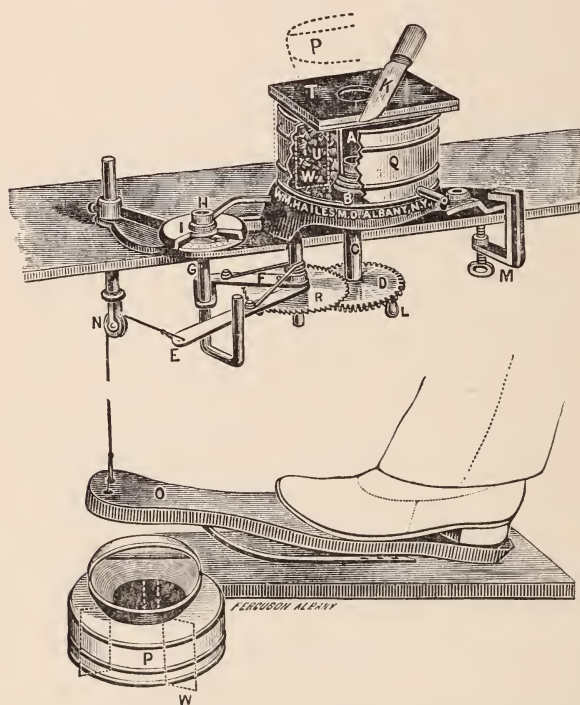
Directions for using the Freezing Poly-Microtome.—When the microtome is used for freezing, remove the glass table, cover the ice-jacket with felt or guttapercha to prevent absorption of heat from atmosphere. Oil the screw and plunger to prevent their becoming fixed by the freezing (too much oil interferes with freezing). Screw cylinder into position on bedplate, close the top with a tightly fitting cork to prevent the entrance of ice, etc. Put on the cover and fill the ice-jacket with finely powdered ice and coarse salt through the hopper opening, and stir the contents by rotating the hopper cover. In a few moments the cylinder will be cooled down to freezing point. Remove the hopper lid and cork, fill the cylinder two thirds full with mucilage acaciæ (B. P.), then replace the cork and hopper lid and stir for a few moments.

When a white, frozen film has formed at the periphery, introduce the specimen into the mucilage in the well of the microtome, holding it against the advancing film of ice until it becomes fixed in the desired position. Then pour in a little mucilage so as to cover it completely. Recork and stir, adding ice and salt, as it becomes necessary, until the specimen is *frozen solid*. It is advisable to turn the microtome screw occasionally during the freezing to prevent its becoming too firmly fixed to the sides of the cylinder.

When perfectly frozen, exchange hopper lid for glass table (which has been previously cooled by contact with ice), then cut in the usual way, working the lever alternately with the cutting. The thickness of a section is controlled by the regulator, as shown on the index plate. The thickness most generally preferred is the $\frac{1}{1200}$ inch, the most desirable range being from $\frac{1}{1000}$ of an inch to

$\frac{1}{1600}$. The temperature at which the best results have been obtained has been when the surrounding atmosphere was about 40° Fah. It works satisfactorily at any season of the year, but requires more experience to work it perfectly in a warm atmosphere, owing to the rapid melting at the surface of the specimen. *All tissues must be frozen solid in order to be cut well.*

Two hundred sections have been successfully cut in a single minute, but a more moderate rate of speed, of about a hundred per minute, is recommended. If during the cutting the tissue should have become partially melted and softened, it must be re-



Description.—*A*, small well, tightly corked, fitting on pyramidal bedplate. *B*, pyramidal bedplate containing different sizes. *C*, micrometer screw. *D*, cog-wheel fitting in pinion of ratchet-wheel *R*. *E*, lever actuating micrometer, served by means of a pawl engaging in teeth of ratchet-wheel *R*. *F*, arm, carrying a dog, which prevents back motion. *G*, regulator, limiting throw of the lever, and consequently governing the micrometer screw. *H*, lever nut for fixing regulator. *I*, index with pointer and scale grade from 1-2400 to 1-800 of an inch. *K*, knife for cutting sections. *L*, knob to turn micrometer screw direct, when the ratchet-wheel is pushed up out of gear. *T*, table of micrometer with glass top to facilitate cutting. *M*, table clamp. *N*, pulley. *O*, treadle. *P*, hopper-lid with wings, *W*, to replace table *T*, when freezing is to be done.

NOTE.—During freezing the table is unscrewed and kept cool by contact with ice. The freezing is facilitated by rotating the hopper lid occasionally; finely powdered ice and coarse salt are added, as required, through hopper opening. This instrument has stood the test of several years of actual work in the laboratory of our school, and has become an actual necessity; by its aid, in ten minutes, if desired, a thousand sections, from 1-1200 to 1-1600 of an inch in thickness, can be readily obtained.

frozen ; this is readily accomplished by disengaging the ratchet-wheel and pinion by pushing it directly upward out of gear. The plunger is made to descend rapidly by turning the micrometer screw directly by knob on cog-wheel. The specimen is then made to descend rapidly. Recork, replace the hopper lid, and stir for a few moments. When thoroughly refrozen, the specimen can be returned to the top of the glass table, with its relations unchanged, and the cutting may be continued as before. The sections after remaining in water for 24 hours should be transferred to : \mathbb{R} Glycerin., aquæ, \overline{aa} \mathfrak{z} iv; acid. carbolic, gtt. iij (\mathbb{M} Boil and filter), or kept indefinitely in alcohol. They remain unchanged for years. Perfectly fresh tissues may be cut without any previous preparation, using ordinary mucilage acaciæ (B. P.) to freeze in. If preserved in alcohol, Müller's fluid, etc., it is necessary to wash for 12 hours in running water ; then place in : \mathbb{R} Sugar (refined or granulated), \mathfrak{z} ij; aquæ, \mathfrak{z} j; \mathbb{M} for 24 hours. Then remove to mucilage acaciæ (B. P.) for 48 hours, and cut in frozen mucilage as already described.

The knife employed is an ordinary strong-handed one, ground nearly flat on the under side, and of a very hard temper. It must be strapped frequently.

The Science and Art of Midwifery. By WILLIAM THOMPSON LUSK, A.M., M.D. New York : D. Appleton & Co., 1882, p. 587.

This work will be warmly welcomed by students and practitioners. It not only better represents the present state of the science and practice of midwifery than its predecessors, but it is the only obstetric treatise of note that has appeared from an American pen in several years. For these reasons it will undoubtedly replace the English text-books now in so general use in our American colleges.

Much of the new matter contained in its pages has been gathered from the recent obstetric literature of the Germans, who, as the author remarks, have in late years occupied a vantage-ground in this department of medicine.

The general plan of the work is entirely similar to that of the excellent manual of Schroeder. The different departments of the subject are presented substantially under the general heads : physiological anatomy, and the physiology, pathology, and treatment of pregnancy, parturition, and the puerperal state respectively.

A clear and satisfactory account of the anatomy of the generative organs forms the opening chapter. The anatomy of the bony pelvis, however, is treated, where it properly belongs, with the mechanism of labor.

Among the earlier chapters, those on foetal development and the diagnosis of pregnancy may be mentioned as eminently valuable and practical ones.

Dr. Lusk abandons the doctrine of superfœtation ; supposed cases of that character he believes to be twin pregnancies in which one foetus has suffered arrest of development.

The mechanism of labor is treated with the thoroughness which its importance demands. Special stress is deservedly laid upon the structure of the pelvic floor and the rôle which it plays in labor.

In the theory adopted from Stephenson, the reader will find a more satisfactory explanation of rotation than in the mechanism generally taught. The theory that the flexion of the head during its passage through the cervix is maintained by the superior pressure of the longer column of amniotic fluid which presses upon the occipital pole of the head, is erroneous hydrostatics. The law of pressure due to mere weight seems to have been confounded with the law of transmitted pressure.

Hohl's method of supporting the presenting part rather than the perineum, is advocated for the preservation of the perineal body. Removal of the forceps before the head passes the vulvo-vaginal ring and rectal expression between the pains are among the measures practised for the same end. Episiotomy is advised when rupture seems otherwise inevitable.

The remarks upon the importance of late ligation of the cord, especially in children born pale and anæmic, should be read by every practitioner of obstetrics.

Of anæsthetics the author says : "The result of my experience has been to make me a warm advocate of their wider employment on the one hand, while proclaiming the necessity for caution on the other." Parturition confers no absolute immunity from the ordinary dangers of chloroform.

The chapter on extra-uterine pregnancy is an excellent *résumé* of the present knowledge of that subject. The chief reliance in treatment in the early months before rupture, is the destruction of the foetal life by means of electricity. A strong Faradic or interrupted galvanic current is passed for about ten minutes directly through the fruit sac without puncture. This operation is repeated

daily, till the tumor begins to shrivel. Prof. Lusk has collected a series of nine cases successfully treated by this method, one in his own practice.

Respecting the use of forceps we note the following statement : " I can only say that with increasing experience my own practice has grown more and more conservative, and my own belief is, that true wisdom requires us to abstain from even trivial operations so long as Nature is able to do her own work without our assistance." The superiority of the Tarnier forceps for all high operations is warmly advocated. This instrument is commended for the delivery of the after-coming head, and for rapid delivery in case of convulsions or similar emergencies. The Simpson instrument is the preferred pattern for general use.

Every skilled operator, we believe, must agree with Prof. Lusk, as to the advantage of applying the blades to the sides of the head rather than to the sides of the pelvis without reference to the position of the head. Transverse positions must of course be an exception to this rule, yet such cases, even, will be best managed by application of the blades in the oblique diameters of the pelvis.

Under the management of pelvic presentations the various methods of breech extraction are treated *in extenso*.

The subject of pelvic distortion occupies three chapters. The relative claims of the different obstetric procedures in deformed pelves are discussed in a judicial manner.

In contracted pelvis with the head above the brim " there is no rivalry between version and the forceps. Forceps in such a case, the author declares little less dangerous than Cæsarean section. Forceps at the brim, in a flattened pelvis, by causing premature flexion, disturbs the normal mechanism. Statistics are adduced to prove that in all but extreme forms of pelvic contraction, where the other elements of the labor are normal, " Nature will do her own work with the least expense of infant-life and with a relatively small maternal mortality."

We believe that a few years will consign the operation of Cæsarean section to a less conspicuous place in obstetric surgery than the author assigns to it. In any case where Cæsarean section is indicated, certainly the operation of laparo-elytrotomy offers an equal chance to the child and a better one to the mother. We cannot concur in the author's opinion that an imperfectly dilated cervix prohibits laparo-elytrotomy. Artificial dilatation may be practised before the operation, or, as in one of Dr. Skene's cases, after completing the vaginal rent. Indeed, the ease of access to

the cervix after opening the vagina offers every advantage for manual dilatation.

The old practice of introducing ice into the uterine cavity in post-partum hemorrhage seems to us a measure to be tolerated only when other means are not available. Again, why use cold water to arrest the hemorrhage from a lacerated crevix when the hot douche is equally efficient?

The chapter on puerperal fever is a valuable statement of the latest views on this subject. The mortuary records show that in New York City nearly one death in one hundred and twenty-seven is due to this disease, and the importance of a thorough knowledge of the etiology of child-bed fever is justly emphasized. In the light of recent views of surgical inflammations, all puerperal inflammations are regarded as fairly attributable, in the great majority of cases, to the action of septic germs.

While the style of the book is, in the main, lucid, a more systematic analysis of certain portions of the work would bring them more easily within the grasp of the student.

Numerous minor errors occur, necessarily inseparable from the first edition of such a work.

The letter-press is clear and neat. The illustrations are numerous and well executed. Many of the figures are from new sources, several from original drawings. [C. J.]

Nervous Diseases: their Description and Treatment.

A manual for students and practitioners of medicine. By ALLAN McLANE HAMILTON, M.D., Fellow of the New York Academy of Medicine; one of the attending physicians at the Hospital for Epileptics and Paralytics, Blackwell's Island, New York City; one of the consulting physicians at the Hudson River State Hospital for the Insane, and male and female insane asylums of New York City, etc., etc., etc. Second edition. Revised and enlarged. With seventy-two illustrations. Philadelphia: Henry C. Lea's Son & Co., 1881.

The first edition of this work appeared in 1878. The author states in the preface to the edition: "It has been my object to produce a concise practical book, and should the satisfaction be ever accorded me of knowing that I have made the subjects of diagnosis and treatment of nervous diseases more simple to my readers than I think they now are, I shall be amply rewarded for the task." In the preface to the second edition, he states that it is "enlarged by nearly one hundred pages, and contains many

new illustrations,—in fact this feature of the book has undergone an almost entire change. The enlargement is a matter of necessity, owing to recent advances in our knowledge of neurological medicine."

We will note briefly the principal changes and additions which have been made in the original work. In the introduction the author very properly says: "I would add a word of caution in regard to the error many of us make in too readily accepting and isolating nervous symptoms as distinct which after all may be expressive of some general disorder. It too often happens that simple digestive disturbances,—cholesteræmia, or perhaps uræmic poisoning—give rise to symptoms that are seized upon as the basis of a distinct nervous disease, and the error is not recognized in time to arrest the true mischief."

To the "Scheme to be used in the Examination of Patients," the author has made the following additions: Under the heading, "Motility": "condition of reflex, excitability." Under "Tremor": "whether evoked by jarring the limb, or by tapping tendons or muscles; accompanied or not by pain; associated or not with rigidity of joints when limb is flexed." Under "Sensation": "appreciation of form." Under "Disorders of Organs of Special Sense": "the existence of color-blindness." Under "Speech": "visual and auditory relations." Thus, as in the first edition, this "scheme," while it includes a great number of points, is too defective in its classification to be otherwise than confusing to the student unfamiliar with the subject.

Under the heading, "Instruments Used for the Diagnosis of Nervous Disease," the author has added several pages on cerebral thermometry, referring principally to Dr. L. C. Gray's valuable observations. He has omitted entirely, however, any reference to Dr. R. W. Amidon's laborious experiments pertaining to the subject.

The percussion hammer, for producing the tendon reflex, is added to the list of instruments. We are surprised at the experience of the author with the Leclanché element for permanent batteries; he denounces it as unworthy of recommendation, and states that "it is dirty, inconstant, and rapidly loses power"; while, according to most authorities, just the opposite of these qualities constitute its chief advantages, if it is properly used.

We like his position concerning statical electricity, in reference to which he says: "Beyond its moral effect upon the patient, especially if there be hysteria, I do not believe that it possesses any advantages over the chemical currents."

We are pleased to see that the author has laid stress upon the use of *large* doses of potassium iodide in a variety of nervous affections. In this respect the book stands in decided contrast to most of the foreign works on the subject, in which, as a rule, the dose recommended is ridiculously small, when judged by the experience of American practitioners.

The author has added to the chapter on "cerebral hæmorrhage" a *résumé*, in eight pages, of anatomical and physiological data, pertaining to localization theories, and refers also to Brissaud's studies of post-hemiplegic contracture, and the tendon reflexes. Three pages, under different headings, have been introduced on cerebral syphilis.

In the division on "Cerebral Anæmia," additional notes on treatment have been made, principally in reference to the author's own experience in the use of nitrous oxide.

A *résumé* is given, in two pages, of Bastian's and Ball's articles on aphasia; or, as the author prefers to term it, "Asemasia." Charcot's and Ferrier's views on the decussation of the optic tracts are also mentioned. A new chapter on "Diseases of the Cerebellum" has been added, consisting of twelve pages. The subject of acute ascending paralysis, has finally been given a place, though it was omitted in the first edition. A separate division on sclerosis of the columns of Goll has also been added.

The author is incorrect in stating that to Lockhart-Clarke belongs the credit of having discovered the central origin of progressive muscular atrophy, as Luys preceded him in this matter.

The article on pseudo-hypertrophic muscular paralysis has been very much improved by a revision of the contributions of Gowers and others.

The author recedes from his former opinion, that fatty food is contra-indicated in this disease, and adopts the opposite view, that the absence of fatty food is detrimental.

The chapter on diseases of the lateral columns has been increased in size from less than two pages to more than twenty-three, the author having incorporated in it a part of his American Medical Association Prize Essay.

In the chapter on "Posterior Spinal Sclerosis" numerous changes and additions have been made. The description of the characteristic pains of this disease is misleading from the statement that they "dart from the feet up the legs and thighs." It

states also that "every pronounced case invariably presents three marked symptoms. (1) Peculiar pains usually seated in the lower extremities. (2) A simple atrophy of the optic disc. (3) An impairment of the reflex function, usually found in the tendon of the quadriceps, or shown in tardy action of the pupils. These symptoms are constant." Ross, on the contrary, states that atrophy of the optic disc occurs in about 30% of all cases of the disease. It is in this chapter that particular attention is given to the tendon reflexes, which, strange to say, found no place in the former edition, either in regard to the particular disease, or, as far as we have been able to note, in reference to any other. The conditions of the pupils in this disease, to which no reference whatever was made in the first edition (not even excepting myosis), are described in the work before us as follows: "The pupils are sluggish, and sometimes entirely insensible to light. They are as a rule both contracted, though they may be unequal. Jackson, alluding to this state of the pupil, which he calls the Argyle Robertson's symptom, states that he believes it to be due to a loss of reflex activity, and but a link in the chain of disordered functions which in the lower extremities is expressed by the absent tendon-reflex." It is left entirely in the dark which of these various conditions constitutes the symptom in question. As a matter of fact, none of the conditions mentioned correspond to the "Robertson symptom," which consists of immobility of the pupil to light, associated with normal reaction to accommodative movements.

The subject of arthropathies receives more attention than it obtained in the first edition.

The author has the merit of being conservative on the subject of nerve-stretching as a treatment for locomotor ataxia.

Concerning the differential diagnosis of this disease, no reference has been made to the spinal form of multiple sclerosis, a condition which is perhaps more frequently confounded with typical systematic sclerosis of the posterior columns than any other.

To the division on alcoholism, is added a paragraph on "acute absinthism," and a new division on "nicotinism" has been well done. The article on hystero-epilepsy has been enlarged by an abstract, and by cuts from Bourneville and Régnard's work, and also by remarks on the "hysterogenic zones." There are additional remarks on the treatment of exophthalmic goitre, and a few lines on the use of hyoscamine. Vigouroux and Granville's "*percutateur*" for the treatment of neuralgia is described, and an

instrument for the same purpose, constructed by the author. He also introduces a method for the treatment of torticollis by which a descending continuous current is applied to the affected muscle, while the opposite muscle has the positive electrode of the primary inductive coil in contact with it. While it is evident that the object here is to apply galvanism to the affected side and faradism to the opposite sides, the author has not explained his views clearly as to what he expects to accomplish by the definite polar arrangement which he recommends.

We are disappointed in finding in this new book, that the author, who has already published a manual on electro-therapeutics, has nowhere mentioned, when considering electro-diagnosis or treatment, the qualitative changes in reactions of muscles to galvanism, found in a variety of diseased conditions. No reference is made to the "degeneration reaction," the importance of which in its relation to diagnosis and prognosis is so well established.

Many cases have been omitted which appeared in the former edition, and also the list of the formulæ at the end of the volume. This, we believe, is a fair statement of what the new edition presents, and in answering the question: To what extent has the author fulfilled in his second edition the object avowed in the preface to the first? we are compelled to reply, that as far as conciseness and system are concerned there is very little improvement upon the first edition. Nor are the corrections and additions as extensive or complete as the subject and the book demand, or as we expected to find. It is unfortunate that a work, parts of which are so well executed, should be so extremely defective in others. In short, the author has not, apparently, devoted the time or exercised the care in the revision of his work which it required, or of which he is capable. And too many of the alterations in the new edition are but corrections of defects which ought not to have appeared in the first. [W. R. B.]

A Practical Treatise on Hernia. By JOSEPH H. WARREN, M.D. Second and revised edition. Boston: James R. Osgood & Co., 1882.

This is a very good encyclopædia—on a small scale—of the modern operations for hernia, and however poorly Dr. Warren appears as an author, all will give him credit for being a good editor, for in this volume of 428 pages we have papers by Samuel Osborn, Esq., Drs. Greensville Dowell, Henry O. Marcy, and Japney, by Claude Bernard, Mr. Spanton, Prof. Annandale, Dr. H.

Braun, and Dr. B. Codman of the firm of Codman & Shurtleff. We have also liberal extracts from Wood, Allen Burns, Gray, Astley Cooper, Birket, etc., etc. In the prefaces so many men in and out of the profession are thanked for real or fancied services done, that one wonders wherein Dr. Warren's own labors appear. The term litholapaxy was spelled *lithopaxy* in the first edition, and naturally we thought it an error of the proof-reader, but when we find it misspelled in the second edition we naturally infer that the doctor does not know how it should be spelled. Sir Henry Thompson certainly could not have "allowed me to witness his operation for *lithopaxy*." In the volume, too, are papers read in Vermont, and Otsego Co., N. Y., introduced without curtailing them of the stereotyped introductions. And that no original work may escape republication, a communication to the *Boston Med. and Surgical Journal* on the removal of tumors from the breast and nates is introduced to illustrate the natural contractility of arteries. The new chapters in this edition are: I, Causation of Hernia; X, Recent Operations for Hernia, in which occurs Mr. Spanton's paper (27 pages), Prof. Annandale's (3 pages), and Dr. H. Braun's on "Czerny's radical cure" (28 pages); XI, Artificial Anus and Wounds of the Intestine, the brief history of which as given may be interesting to the student of medicine; XIII, Hydrocele and Varicocele, inferior to those given in text-books on surgery; XIV, Observations on Hernia, being annotations and errata by which he can claim for this a new edition and thus save labor; and XV, Résumé and Clinical reports.

In the body of the work are given three very fair plates, reproduced from Bourguery and Blaudin, all inserted into the *second* edition and having no connection whatever, by reference, with the text. As it is, they are placed at convenient distances apart, so that if one should weary reading he can study pictorial anatomy.

The profession at large is indebted to Dr. J. Henry Davenport for a knowledge of the Heaton method of treating hernia by injecting the *canal* with white oak bark. Dr. Davenport edited Heaton's book in 1877. Dr. Warren did not operate at all until *July 10, 1879*, and did not publish his book until 1880. And yet we are to give Dr. Warren credit for making known to the profession the Heatonian method, or, as he prefers now to call it in his new chapter of annotations, the "Pancoast operation for the cure of hernia by the subcutaneous method."

Inasmuch as "my *improved* operation would seem to be more

acceptable to the better and greater part of the profession than previous operations, if I can judge by the letters of congratulation I receive from distinguished surgeons of this and other countries," it may be well to look into the difference between Heaton's method and the Warren modification. The one used a plain syringe with brad-awl needle, perforated transversely near the extremity ; the other uses a spiral needle attached to a syringe, so arranged that the contents can be seen and the contents of the injecting fluid measured, like the barrel of an ordinary hypodermic. The piston works by a spring, and there is at the lower end of the barrel a concave semicircular handle. This is much more cumbersome-looking and more complicated than the Heaton syringe, and practical surgeons in New York, at least, tell us it is inferior to Heaton's. Warren adds alcohol, sulphuric ether, and tinct. veratri viridis to Heaton's formula. Both occasionally get "inflammation, swelling, and abscess."

Dr. Warren would have us believe that he is the more honest of the two ; indeed, the care he takes to assert and to re-assert throughout the whole volume that *he is honest*, makes one feel embarrassed in accepting any statements from a man who feels it necessary to go about placarded "I am honest." In chapter VII we are told : "When this operation is attempted upon persons in poor and indifferent health, or of great delicacy, enfeebled by age, or a broken constitution ; upon those who have lived lives of intemperance and debauchery, or who are suffering from syphilis or scrofulous affections ; upon those living in crowded and unhealthy places, as in the filth and poverty of a great city ; upon those in hospitals and public institutions, as almshouses, jails, places of detention, or prisons ; or upon poorly-nourished, and anæmic, and upon dispensary patients, the prognosis will be very unfavorable and the chances of success very small and uncertain." That sentence reads like a plagiarism from the common prayer book, and one naturally cries out, "Who, then, Lord, will be saved ?"

Let us hope that the next edition will be expurgated of such sentimentalism, will be arranged in a style more suitable, will show less of this haste and more work. There is certainly material enough in the book to make a volume that will do credit to the classic city whence it comes, and will hold out encouragement to the poor as well as to the rich.

[V. P. G.]

Sixth Annual Report of the Managers and Officers of the State Asylum for the Insane at Morristown, New Jersey. For the year 1881.

Another year brings with it an annual visitor with the above title. It is a neatly printed pamphlet of 32 pages, and purports to supply the citizens of the State with information concerning its conduct and the condition of its family of 586 unfortunate insane persons. First, the managers' report, which covers less than two pages, tells us that they offer to his Excellency, the Governor of the State, the reports of the superintendent, steward, and treasurer, which are appended, and refers the reader to them. It then tells us that an inventory of the *personal property* of the institution shows that its value is \$99,473 14, and that the managers have maintained an "effective inspection of the asylum" by "weekly visits of one or more" of them, and that "memorandums" of such visits "have been made in a book kept for that purpose." They want \$30,000 for improving the grounds, and refer to a gift of instruments of precision, and compliment the superintendent for "suggestions" on the "employment of the insane," which "evinced great care in their preparation," etc. We always look to the report of the superintendent, hoping to find something fresh in the way of discovery within the range of cerebral pathology, and if not in that particular line, something encouraging in the line of treatment and its results. The notion of employment "involving muscular exercise and some concentration of attention" being "advantageous as a means of improving the physical, and through it the mental state," is as old as Hippocrates, and as well known to every freshman in a college roster as it is to the learned superintendent of this asylum. But we look over the pages of the report in vain to find a word that sheds any light on pathology, or that points to any plan for preventing or curing insanity, while we are confronted with one humiliating statement, that out of 773 cases that had been under treatment during the past year, there are only 35 "discharged recovered." Also, that out of 1,186 insane persons "received from opening of asylum, August 17, 1876, to November 1, 1881," there have been "discharged recovered, 142."

It is usual to append a table of causes, but the poor ignorant public, who are told so constantly that insanity is on the increase, and that we are all likely to drift in that direction, are left without any notice of what causes lead to insanity, that we may avoid them. The superintendent is, however, not en-

tirely devoid of interest in the unfortunates over whom he is placed. He regards as a subject of "great importance as connected with the facilities of the institution for carrying out successfully its great and benevolent purposes," that the design proposed for laying out carriage drives, approaching the doorways, a central avenue, fifty feet wide, with stone-paved gutters, shade trees and walks, and an ornate gate lodge, etc., should be executed, but knows not whence the money is to come. It does not appear that the fine climate of Morris Plains, the natural woods, and pastoral views, and flowing brooks from the hillside, which furnish such an abundant supply of wholesome water, are sufficiently attractive or invigorating for the semi-conscious multitude who inhabit the palace, without \$30,000 being expended to mar the slopes that nature has made, and decorate (?) with stones and gravel the plains which are better as the gift of heaven than as curious art would make them. But the "Instruments of Precision"! It is a shame to the honored State, which has poured out of its treasury not far from three millions of its money for this institution, that not a scientific step has been taken, not a scientific fact discovered, not a scientific treatment proclaimed since the asylum was organized. The people who give the money have a right to know something about insanity: its causation and symptomatology; how it can be prevented, and how cured. And now private benevolence has sent a microscope to be used in the line of pathological research. Has it been used? Has Loring's best ophthalmoscope displayed any lesion in the cerebral structure or its meninges? and have the other instruments been employed in the field of discovery? Has any thing been done? These are questions that intelligent laymen are beginning to press home to those who should answer without the asking. There is beauty without the institution—costly beauty; but within, the results are not equal to some of our county almshouses, where no beauty is. Public sentiment has been silent too long. The humdrum method of dealing with insanity by trained routinists, who turn neither to the right nor the left, has had its day. The time is at hand when there will be a demand made that cannot be resisted—a demand, first, for scientific knowledge; next, for the application of such knowledge within the walls of lunatic asylums; and then for results that shall not make us bow our heads with shame. [J. P.]

ORIGINAL OBSERVATIONS

SARCOMA OF THE CEREBRUM, ILLUSTRATING THE DOCTRINE OF LOCALIZATION.

By GEORGE L. PEABODY, M.D.,

MEDICAL REGISTRAR AND PATHOLOGIST TO THE NEW YORK HOSPITAL.

The following case is published merely as additional evidence of the truth of the theory of cerebral localization.

J. W., æt. 50 ; England ; married ; bookkeeper ; was admitted into the service of Dr. William H. Draper, at the New York Hospital, October 2, 1881.

He says his parents both died suddenly, they having been found dead in bed. They had nine children, of whom one only is dead ; he having died of some liver disease. The others, excepting the patient, are healthy. The patient admits having had gonorrhœa once, but emphatically denies syphilis. He has been a drinker of beer and whiskey. Had erysipelas in childhood, which accounts for certain scars on one of his legs. Has never had rheumatism. Gives no history of renal, pulmonary, or cardiac disease, or of injury. Last winter, after a shower-bath, he experienced a sense of numbness in the left foot, from the great toe to the ankle, which persisted for some time, but to which he paid no particular attention. In the early part of this year he felt somewhat run down in general health, but with no very definite symptoms. For this he travelled. Last June (four months ago), in the early morning, while still in bed, he had, on awaking, a dull pain about the left hip joint, and a sensation as if all were gone below that on the left side. Was able at this time to move the left leg. Soon thereafter there began a spasmodic up-and-down jerking of the whole pelvis ; excruciating pains in the left foot, leg, and thigh ; and

sudden, violent, clonic spasm of the whole left lower extremity (the thigh being suddenly and completely flexed upon the abdomen). The pain seemed then to creep up to the left arm, and to the neck. After this he lost consciousness, and remained unconscious for an hour. On regaining consciousness there was no aphasia, but his tongue was terribly bitten. After the attack he regained completely the power in his limbs. The patient then remained in good health up to two or three weeks ago, at which time, under the same circumstances and surroundings, he had a second attack, which differed from the first only in that he did not lose consciousness and did not bite his tongue. He regained power on the same day, and attended to his business as usual. Six days ago he had a third attack exactly like the second, except that it began by clonic spasms in the left arm. During the next two days he went to his place of business as usual. During the following two days he was only able to drag himself about. After this time, had to give up, being able to get out of bed only with assistance. Loss of muscular power has daily deepened.

There has been no cincture pain, or sphincter trouble ; no pain in the head, and no aphasia ; also no urinary or alimentary disturbance.

On admission, pulse, respiration, and temperature are normal. He is fairly nourished; there is no œdema; no loss of cutaneous sensibility ; considerable though not complete loss of power in left upper and lower extremities. Trial by dynamometer shows left hand 0, right hand 80. There are no ocular symptoms, and no deviation of the tongue. Heart, lungs, liver, and spleen are normal.

During the month following his admission to the hospital, he had, at intervals of a few days, marked convulsive movements of the left side of his body and extremities. He was during this time given only bromides of potassium and ammonium, with an occasional small dose of morphine. It was noticed that after each of these spasmodic attacks the patient would lose for several hours the little remaining power of his left extremities. At the beginning of his second month in hospital (November 9th), he was given mixed treatment, which consisted of hydrarg. biniodid., gr. $\frac{3}{4}$, and potass. iodid., grs. x. This dose was given three times a day. During the month of November there was no marked change in his condition. The muscular spasms became rather more frequent, and his general nutrition suffered perceptibly.

Early in December his convulsions became more severe and more general, and then, for the first time since he came under observation, they were accompanied by periods of complete unconsciousness, which lasted several hours. At this time, too, he began to complain of intense pain over the right hemicranium. Occasional rigidity of the right half of the body was now noted. On December 17th it was noted that the right half of his body was quite stiff, and that spasmodic contractions of the muscles of the right hand and forearm occurred. The face became distorted and drawn toward the right side, and his tongue deviated toward the right. He now began to lie in a dull, lethargic condition during much of the time, but he could be roused to answer questions about himself. At this time he passed urine and fæces involuntarily. There was no strabismus at any time. From time to time now his temperature became elevated; up to this time, however, never reaching 101° .

On December 20th he became comatose, and his temperature rose to 104.1° F. From this time his temperature steadily rose until he died, comatose, of heart failure, at 10.30 P. M., on December 21st, with a temperature of 108.7° F.

Autopsy 12 hours after death :

Body emaciated. Rigor mortis well marked. There is no œdema. There are superficial scars on the external aspect of the left leg; none on the penis, or elsewhere. The following measurements of the limbs were taken :

Circumference of thighs at middle, R., 36.40 cm.; L., 31.40 cm.

Circumference of legs 10 cm. below patella, R., 27. cm.; L., 25.80 cm.

Circumference of arms at middle, R., 22 cm.; L., 17.40 cm.

Circumference of the forearms 8.50 cm. below tip of olecranon, R., 22.20 cm.; L., 18.80 cm.

Rigor mortis is less firm on the left side than on the right. There is a small superficial bed sore over the upper part of the sacrum.

Brain.—There is marked flattening of the convolutions generally, which is more marked on the right side. The pia mater is whitened and thickened and firmly attached to the dura mater over the posterior part of the first frontal convolution on the right side. After removing the brain from the calvarium and placing it upon its base, it is noticed that the left hemisphere falls away from the median line, partially collapsing as usual. The right

hemisphere does not collapse at all, its highest point (fissure of Rolando) being on a plane 1.75 cm. higher than the corresponding point of the left. There is a bulging of the anterior $\frac{2}{3}$ of the right hemisphere inward across the median line. There is distinct fluctuation obtained over the anterior half of the right hemisphere. The brain was then put into chromic acid for hardening, it being merely noted in addition that the arteries at the base are normal.

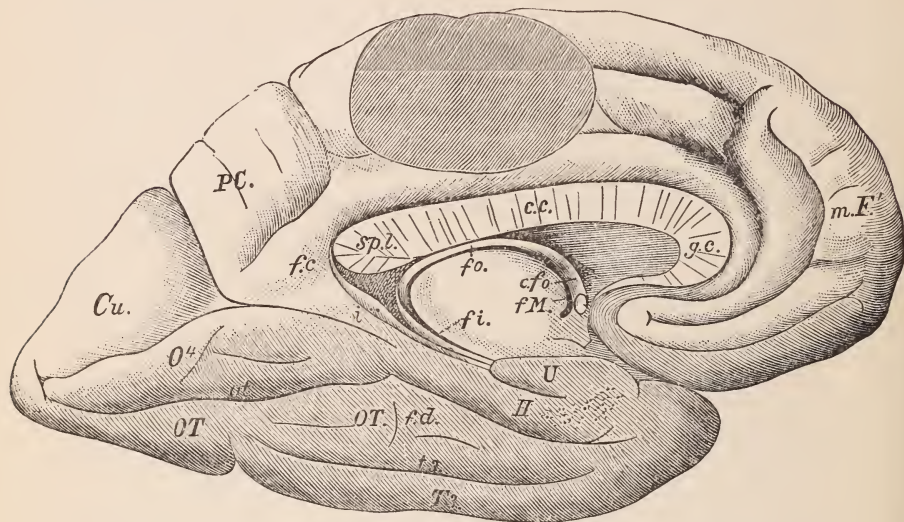


FIG. 1.—View of inner surface of left (should be the right) hemisphere, after Schwalbe. Shaded spot indicates the location of the tumor.

On subsequent examination of the brain a tumor was discovered in the right hemisphere, ovoid in shape, with a long antero-posterior diameter of 5.60 cm. and a short diameter of 3.70 cm. It begins in the white matter of the paracentral lobule just at the terminal portion of the fissure of Rolando, and lies beneath the upper half of the ascending frontal convolution and the posterior part of the first frontal convolution. It has invaded and destroyed much of the gray matter of these two convolutions, but has not reached any of the gray matter elsewhere.¹

Microscopically the tumor was found to be a sarcoma which contained both round and spindle cells.

The other viscera were substantially normal.

¹ Compare a similar case published by me in *Journal of Mental and Nervous Disease*, 1881, No. 3, case 2. p. 515.—[EDITOR.]

The cuts given show approximately the position of the tumor.

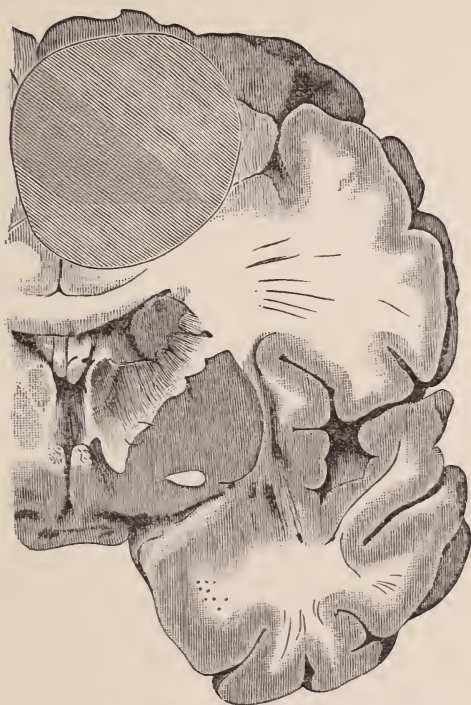


FIG. 2.—Transverse vertical section of right hemisphere, after Fig. No. 4 of Bitot. Shaded spot in upper part of figure indicates the location of the tumor.

URÆMIC DELIRIUM AND COMA AT A VERY EARLY STAGE OF INTERSTITIAL NEPHRITIS.

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The chief points of interest in this case are : (1) the onset of the symptoms with delirium ; (2) the mental worry which preceded the attack—a prominent factor in some instances ; (3) the apparently normal state of the kidneys, which gave evidence of changes only on microscopical examination.

J. W., aged 44, a large, powerfully-built man, railway foreman, was admitted into the general hospital, May 6th, with delirium. For past fifteen years had been temperate ; prior to this had taken a good deal of alcohol. Up to present attack had enjoyed

good health ; his wife states that she had noticed of late that he got up at night to make water, and he passed rather more than usual. For about a week he had been greatly worried, as a strike had occurred among the men in his department. On the morning of the 4th he had a chill, and felt unwell all day. On the 5th, though still ailing, he went to work, but returned in the afternoon complaining of soreness over the whole body, headache, and chilly feeling. In the evening he became delirious and was noisy and excited all night ; could not be kept in bed, but walked about incessantly talking and directing his men at their work. On the 6th the delirium persisted, and in the evening he was brought to hospital. On admission the temperature was 100° , pulse 120. He passed a very restless night, and was with difficulty kept in bed. In the morning (7th) he was quieter ; temperature 100° . When seen at the mid-day visit was quieter, but did not answer questions intelligently. Face flushed, venules on cheeks and nose dilated. Pupils slightly contracted, react to light ; ophthalmoscopic examination of eyes negative. No special symptoms in chest or abdomen. No dropsy, nor œdema of ankles. Heart's impulse not forcible ; apex in normal position, but difficult to feel ; pulse full in volume, tension plus ; radials not stiff. Urine was passed in bed ; that drawn off with catheter, high colored, sp. gr. 1039, highly albuminous, with numerous finely granular casts, many of unusual length. Toward the afternoon he slept. In the evening was very torpid ; did not know his wife. Temperature normal. On the 8th, after a quiet night, he was very drowsy, roused with difficulty ; pulse 120, temperature $99\frac{1}{2}^{\circ}$. Pupils of medium size, react slowly. Passed 24 oz. of urine ; same characters as before noted. Toward the evening he became deeply comatose ; the respirations increased. He was bled to 20 oz., with the effect of reducing the rapidity of pulse and respirations. Pupils dilated ; temperature rose to $103\frac{1}{2}^{\circ}$. The coma increased, and death took place at 3 A. M. on the 9th. The treatment consisted in bromides and chloral in the early stage ; purgatives, pilocarpin, and vapor baths.

Autopsy.—*Brain* : Arachnoid turbid at base and over the sulci ; much serosity about the membranes, which stripped off very easily. Several slight ecchymoses in gray matter of right hemisphere ; one at top of ascending frontal gyrus was the size of a small pea. Ventricles contain a moderate quantity of fluid ; walls not softened. On section, substance of the organ not specially moist. The arteries at the base not atheromatous. *Heart*

weighed 382 grammes; valves healthy, muscle substance of good color; walls of left ventricle measured from 15–18 mm.; chamber, 8.5 cm. from apex to aortic ring. Aorta presented a few scattered patches of atheroma. Nothing of special note in *lungs, spleen, stomach, or intestines*. *Kidneys*: right, 190 grammes; left, 175. Capsules detach readily and leave smooth surfaces; nowhere granular. Organs cut with moderate firmness; cortices not diminished; medullary rays very distinct; intervening vascular regions with the tufts injected. Arteries at bases of pyramids not unusually prominent. Pyramids look normal. Renal arteries not atheromatous. Altogether, the *macroscopic* appearance of the organs did not appear to substantiate the diagnosis of uræmia which had been made. Beyond a slight increase in firmness the glands certainly did not present appearances which would have attracted further attention had not the symptoms demanded it.

On microscopical examination the only striking change was in the Malpighian tufts, a number of which were found atrophied and surrounded by an increased growth of fibrous tissue. In some, where the process was far advanced, the tuft was converted into a small homogeneous mass, without nuclei; in others, portions of the tuft appeared normal. There did not appear to be any special proliferation of epithelial elements within the capsule, but there was a very general thickening of the delicate zone of fibrous tissue about even healthy-looking ones. In the neighborhood of several atrophied tufts there was a small-celled or nuclear growth separating the tubules. The small arteries presented decided hypertrophy of the muscle elements, particularly in the circular coat; no hyaline degeneration of the intima. The epithelium was everywhere healthy-looking, distinctly granular, but not swollen; and there were no collections of epithelial *débris* observed in any of the tubules. Except in the vicinity of the atrophied tufts no increase in the intertubular connective tissue was noticed. In the pyramidal portion some of the tubules presented finely granular casts.

A REPORT OF ANATOMICAL ANOMALIES.

By A. H. P. LEUF, M.D.,

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It is my purpose to present, as briefly as possible, some anatomical anomalies, that I have observed in the dissecting-room of the Long Island College Hospital from Oct. 1878 to Feb. 1881. I have arranged these facts in the order adopted by Gray in his anatomy.

BONES.—*Atlas.* In one specimen the vertebral groove on the right side was converted into a canal. In another was seen nature's unsuccessful attempt to make a similar change on both sides, the centre of the superior arch, however, being defective. On both sides of this bone, between the incomplete vertebral canal and the transverse process, was situated an incomplete foramen, 3 centimetres in diameter, and probably for the passage of the occipitalis major nerve. Henle says: "In one instance the *nervus occipitalis major* passed through a bony canal behind the transverse process."¹

Another subject had its left vertebral groove transformed into a canal, and the spines of the 3d, 4th, 5th, and 6th cervical vertebræ were remarkably bifid, each ending in a long and a short process. The average length of the long one was 1.5 cent., and that of the other .5 cent. The long spines of the 3d and 5th were on the right side, and those of the 4th and 6th on the left side of the median line. A neck containing such spines would be very apt to mislead one in an examination, and might prove the cause of an unfavorable prognosis on the part of the physician, and considerable annoyance and unnecessary restraint to the patient.

Sacrum.—The posterior arch of the first segment appeared as though its right half had been tilted downward and inward. There were two very distinct spinous processes, although the posterior arch was complete; the left one being longer than its fellow, which it overlapped, and cylindrical in form, while the right was flat, broad, and quadrilateral. The right superior articular surface looked downward, while the left did not.² The lower segment of the sacrum was firmly united to the upper piece of the coccyx, as were also the cornua of both bones.

A right *temporal* had a large open cleft in place of the Glasserian fissure, being 2 cent. long and 1 cent. broad at its widest or middle portion, and 5 cent. at its narrowest or external part. It seemed to be due to an arrest of development. Henle mentions this occurrence.³

¹ "Einmal verlief der N. occipitalis maj. durch einen knöchernen Canal hinter dem Querfortsatz." Henle: Dritte Auflage Knochenlehre, Erster Band, Seite 51.

² I failed to find any mention of this anomaly of the sacrum.

³ "Oft ist die vordere Wand des Gehörganges in einer kleineren oder grösseren Ausdehnung durchbrochen entweder in Folge mangelhafter Verknöcherung oder durch abnutzung. Cassebohm: Tract. de aure humana, Hal. 1734, p. 28, Taf. i, fig. 2 r. Dieterich: a. a. O. S. 10, fig. 1 bb. Hyrtle: Spontane Dehisc., S. 6. Retzius: Schmidt's Jahrb., 1859, Hft. 11, S. 153." Henle: Drit. Aufl., Erst. Band., Knochenlehre, S. 156.

The left *ulna* of one subject had the olecranon process replaced by a sesamoid bone equalling it in size, with no other muscular attachment than the triceps. The same subject also presented a *fibula*, of which the shaft of the bone bent inward so as to touch the tibia. This bone was not deficient in calcium salts and all the other bones were well formed. The subject was a male aged about 45 years, and there was no evidence of fracture in the bent bone.

MUSCLES.—*Digastricus Accessorius*. Two muscular slips, each 3 cent. long and fusiform in shape, which arose from the fascia covering the hyoid bone at the attachment of the digastric. From thence both muscles passed upward and inward, half way to the chin, where they decussated to the extent of 1.5 cent., the fibres of the right side being superficial.

Tensor Fasciæ Thoracis.—This was a continuation of the tendon of the sterno-cleido-mastoid downward for about 2.5 cent., where it expanded into a flat muscle, passing for insertion to the fascia over the 5th, 6th, and 7th costal cartilages. The muscle was about 10 cent. long, and its lower, widest part 3 cent. broad. This specimen was bilateral. I have also seen four unilateral specimens almost identical with the above. Two were situated on the right side and two on the left. Henle describes this muscle under the name "musculus sternalis," and remarks: "*Hallett* found it once in every 15 cases. *Gruber* in 100 bodies 5 times; 3 times bilateral and 2 times unilateral. *Turner* in 650 bodies 21 times; 7 times unilateral, 9 times on both sides and 5 times oblique, from one side to the other. *Wood* in 175 bodies 7 times, and of these only one was bilateral."

Costo-Coracoideus.—This muscle arose from the upper border of the sternal end of the fifth rib to the aponeurosis of the short head of the biceps, 3 cent. below the coracoid process (left side).

Chondro-Coracoideus (left side).—A muscular slip as thick as an ordinary slate pencil, passing along the lower border of the pectoralis major. It arose from the aponeurosis of the hyposternal notch and from the seventh costal cartilage, and was inserted into the conjoined tendon of the biceps and coraco-brachialis, 2.5 cent. below the coracoid process.

Pectoralis Minor.—This muscle had a very distinct origin from the 2d, 3d, and 4th ribs, and was so on both sides of the same subject. The coracoid attachment was normal.

Tendo Accessorius Subclavii. The subclavius arose normally

¹ *Henle*: Dritte Aufl., Erst. Band., Muskellehre, S. 98, 99.

from the first rib and had a small narrow attachment to the clavicle at the middle of its under surface, where it became tendinous and passed downward and outward, over the axillary artery, for insertion into the coracoid process. Henle refers to Haller (de corp. hum. fabr. vi, 77) while mentioning "an accessory tendon to the coracoid process" from the subclavius.

Musculus Basis Axillæ (left side).—This consisted of a muscular slip passing from the upper border of the latissimus dorsi forward across the axilla, in front of the axillary vessels, to be inserted into the under surface of the pectoralis major near its insertion. I noticed a similar slip, also of the left side, in a fœtus at term, which arose from the upper part of the anterior border of the latissimus dorsi, and crossed the base of the axilla to the middle of the lower border of the pectoralis major.

Gray, in his work on anatomy, cautions surgeons to bear this in mind when operating in the axillary space, as it might otherwise mislead them.

Triceps Flexor Cubiti.—The third head arose from the internal condyloid ridge, between the insertion of the coraco-brachialis and a point about 2 cent. above the internal condyle.

Extensor Carpi Radialis Brevior.—One half of this muscle was derived from the belly of the extensor carpi radialis longior. This head is called by Wood *extensor carpi intermedius*.

Musculus Flexor Communicans.—This was a musculo-tendinous slip from the middle of the belly of the flexor sublimis digitorum to the annularis tendon of the flexor profundus. This slip was 1.25 cent. in length, and gave origin to the second lumbrical muscles, counting from the ulnar side.

Flexor Pollicis Accessorius.—I have, on eight different subjects, seen a small muscular slip, about 7 cent. long and 15 mm. in its largest diameter. It was fusiform in shape, and had a tendinous origin from the under surface of the belly of the flexor sublimis digitorum, about 2 cent. below its coronoid attachment, and was inserted into the flexor longus pollicis where the muscular belly became tendinous. This slip is not identical with that head of the flexor longus pollicis which is described by Dr. J. F. Walsh, in the *Annals of the Anat. and Surg. Soc.*, vol. ii, pp. 458-9.

Extensor Ossis Metacarpi Pollicis Accessorius.—This muscle arose from the extensor carpi radialis longior, midway between the internal condyle and insertion of the pronator teres. It became tendinous at the insertion of the pronator, like the muscle

from which it was derived, and was inserted into the base of the first metacarpal bone, above the extensor ossis metacarpi pollicis.

Quadratus Femoris Secundus.—A quadrilateral muscle, about 8 cent. long and 4 cent. wide, which extended from the tuberosity of the ischium to the back of the femur, below the linea quadrati. This muscle was separated from the quadratus femoris proprius above, and the adductor magnus below, by an interval of fully 1 cent., which was filled with loose connective tissue.¹

ARTERIES.—*Left Common Carotid*. In two instances this vessel arose from the *innominate*, and consequently the *aortic arch* was abnormal in giving rise to only two vessels, *i. e.*, innominate and left subclavian.

Right Subclavian.—In one instance this vessel was without a superior intercostal branch, but it gave off two others that were abnormal, 1 cent. external to the thyroid axis. They passed upward to the deep structures of the neck. They were 12 mm. apart at their origin, and measured 3 mm. in diameter.

I observed a left subclavian without a thyroid axis in a fœtus at birth. The vertebral was normal. The second branch ran upward and outward, between the anterior and middle scaleni muscles, to pass in front of the seventh cervical nerve, .5 cent. external to the transverse process of the seventh cervical vertebra. From this point it passed behind the fifth and sixth cervical nerves to reach the levator anguli scapulæ, in which it terminated. The *transversalis colli* passed between the two primary trunks of the brachial plexus. The *suprascapular* passed through the lower primary trunk of the same, causing it to split in its passage through it.

Right posterior temporal passed behind the ear and under the retrahens aurem muscle. The anterior temporal was normal.

Right Axillary.—This vessel gave off four large muscular branches and two thoracicæ alares. A common trunk was given off at the axillo-brachial junction, which divided into superior and inferior profunda. The *thoracica longa* of the same side gave origin to four large *thoracicæ alares*, and in the same subject and on the same side the *anterior circumflex* and *superior profunda* arose by a common trunk from the axillo-brachial junction. I

¹ I have sought in vain for a mention of this anomaly in any of the books. This muscle seems to be anomalous in only one respect, according to the books, *i. e.* in its occasional absence.

failed to see a *superficialis volæ* in the same limb, but instead noticed a branch of the radial, which passed around the back of the first metacarpal bone and terminated in the adductor pollicis. This branch was about 3 mm. in diameter.

The left arm of one subject presented the following: *Posterior circumflex* and *superior profunda*, by a common trunk, from the beginning of the brachial. *Anterior circumflex* and *inferior profunda*, by a common trunk, from the brachial, 2.5 cent. below the preceding. The *anastomotica magna* arose from the *inferior profunda*.

The *brachial* divided 1.25 cent. below the internal condyle and immediately above the bicipital fascia. The *radial* passed under the fascia, and its upper 2 cent. was overlapped by the pronator teres, and was subcutaneous below this point. The *ulnar* was quite superficial. It was covered anteriorly by the *palmaris longus*, 5 cent. below the internal condyle. It descended to the inner and back part of the forearm, while it lay between the *flexor profundus digitorum* and *flexor carpi ulnaris*, slightly overlapped by the former, from 2 cent. below the middle of the forearm to within 2.5 cent. of the pisiform bone.

Anterior Interosseous.—This was not as deep-seated as usual, as it was covered by only three muscles, *i. e.*, radial head of *flexor sublimis*, the pronator teres, and tendon of the *flexor carpi radialis*. It was almost as large as the ulnar. Opposite the wrist it divided into two palmar branches, each of which divided into two digital branches opposite the metacarpo-phalangeal articulations. The two branches on the ulnar side supplied the contiguous sides of the *medius* and *annularis*, and the other two those of the *medius* and *index*.¹ The ulnar supplied one and a half on its side, and the *superficialis volæ* was very large, and divided into *princeps pollicis* and *radialis indicis*.

The *median*, in one instance, arose from the brachial, 1.5 cent. above the bifurcation, and accompanied the median nerve. It passed downward between the *flexor longus pollicis* and *flexor profundus digitorum*, and behind the *flexor sublimis digitorum*. This vessel was enlarged to the size of the radial. In its passage under the annular ligament of the wrist it hugged the trapezium, and in the palm formed the superficial arch. It sent an independent slip to accompany a digital branch of the median nerve to the radial side of the ring finger.

In another case the median artery was also enlarged, and sup-

¹ I could find no mention of this anomaly.

plied three and a half fingers on the radial side, and the ulnar one and a half on its side. The radial met the deep branch of the ulnar to form the deep palmar arch.

The *aorta* referred to above, in relation to the anomalous origin of the left common carotid from the innominate, was also minus the right eighth intercostal artery.

The *cœliac axis* and *superior mesenteric* arose from the same opening in this vessel ; or rather, the latter arose from the former. The two *renals* were given off only 2 mm. lower down. The left *suprarenal* arose opposite the *cœliac axis*, and the right came from the *renal*.

In another subject the *aorta* was very much enlarged above, measuring 6 cent. in diameter at the arch, and gradually tapering from this point down to the bifurcation, where the calibre was normal. At its upper part this vessel sagged down to the extreme left of the spinal column ; at the diaphragm it was in front and lower down to the right of the vertebræ, when it again turned to the left and bifurcated opposite the middle of the fourth lumbar vertebra in the median line.

The *common* and *external iliacs* were related quite unusually in this subject. These vessels, exactly the same on both sides, instead of following the inner border of the *psaos magnus*, dipped down into the pelvis, so that their lowest points rested on the ischiadic spines. The appearance of the vessels was like a short loop of stiff rope. The relations of both to *Poupart's* ligaments, as they passed under them, were normal. This anomaly should be borne in mind by any one who attempts to ligate this vessel, as otherwise the operator might experience considerable annoyance, and the patient much danger.¹

Both *obturator arteries* of this subject arose from the inner side of the femoral artery, 1 cent. below *Poupart's* ligament ; thence going forward along the inner side of the vein passed through the femoral ring, hugging the edge of *Gimbernats* ligament. After leaving the ring both vessels pursued their normal course.

In another subject both *obturator arteries* arose from immediately underneath the ligament of *Poupart*, so that it was difficult to tell whether they came from the external iliac or femoral. Both

¹ *Luschka* noticed a similar anomaly, but less in degree. *W. Krause*, while speaking of the *iliacs*, says : " Sie bildet am oberen Rande der Incisura ischiadica major eine nach abwärts convexe Schlinge, aus welcher die Aeste der fehlenden A. hypogastrica direct entspringen (*Luschka*). " In other words, " It forms a downward convex loop on the upper border of the great sciatic notch, from which the branches of the missing hypogastric artery directly arise."

vessels passed downward from their origin, forming a loop about 1 cent. long. On both sides this loop and its returning branch were situated between the femoral artery and vein, and passed through the ring, hugging the pectineal ridge behind the vein, and close to the artery. Thereafter the course and relations of both vessels were normal.¹

The right obturator artery of another subject arose from the femoral at its beginning, and entered the pelvis at the inner side of the ring. The obturator of the left side differed from its fellow only in origin, which was from the lower end of the external iliac.

I have also seen several unilateral anomalies of this nature, and I distinctly recollect that they were not different from those above, and occurred on both sides.

Accessory External Circumflex (right side).—This vessel arose from the *superficial femoral* 3 cent. below the origin of the profunda, and was .4 cent. in diameter. It supplied the quadriceps extensor.

In one instance the left *profunda femoris* was without internal or external circumflex, but 3 cent. below its origin this vessel gave off a branch 1.5 cent. long, dividing into two ascending branches to the iliacus and psoas muscles, and a large descending branch to the vastus externus; thence continuing outward to the tensor vaginæ femoris, 2 cent. lower down, the profunda sent off another branch, which ran downward and outward, and divided into anterior and external; the former going to the middle of the rectus femoris, and the latter to the middle of the vastus externus.

NERVES.—The *descendens noni* was found within the carotid sheath, and in front of and between the vessels. The *thyrohyoid* branch arose from it, opposite the middle of the thyroid cartilage, and was compelled to pierce the sheath to reach its destination.

The left *recurrent laryngeal* arose from the pneumogastric opposite the transverse process of the third cervical vertebra, instead of opposite the left subclavian artery. Of this I could find no mention.

The right *median* was found 2.5 cent. below the beginning of the brachial artery, and the *musculo-cutaneous* of the same limb failed

¹ Of this anomaly I have also failed to find any mention. It strikes me that the occurrence of the loop must be very rare. Dr. W. Krause (*loc. cit.*) says that the obturator, when arising from the femoral, always passes through the femoral ring to the inner ("medial") side of the femoral vein.

to pierce the coraco-brachialis muscle, and passed across the middle of the median cephalic vein, in front instead of behind. Hence, care should be taken in the operation of phlebotomy, so as not to cut this nerve if it happen to bear this abnormal relation to the vein. I was unable to find any account of these anomalies of the median or musculo-cutaneous.

ARCHIVES OF MEDICINE.

Original Articles.

THE CARCINOMATOUS METAMORPHOSIS.*

By A. W. JOHNSTONE, M.D.,

DANVILLE, KY.

IN the winter of 1880-81, while working in Dr. Heitzmann's laboratory in New York, I examined four lymph glands, three of which showed that there are several stages in the formation of cancer.

The first came from a gentleman 48 years old, on whose prepuce a few small nodules had grown.

When his surgeon heard that they were cancerous he amputated the penis, but six months afterward, two lymph glands, one the size of a hazel-nut and the other as large as a pea, were found in his right groin.

Both were extirpated, and although two years had then elapsed, the patient was still perfectly well.

The second gland came from a male inmate of Charity Hospital, N. Y., aged 42. He had a cancer of the throat that bled so freely as to necessitate the ligation of the right carotid. This was followed by excision of the tumor and the removal of an enlarged lymph gland from the posterior maxillary region. The man died a few days after the operation, and at the autopsy small abscesses were found in the lungs and some yellowish nodules in the liver and

* Read before the Kentucky State Medial Society, April 6, 1882.

kidneys, which the microscope showed to be secondary cancer in its earliest stage of development.

A gentleman of over 50 years furnished the 3d specimen. About a year before he was operated on for cancer of the skin on the left leg. Shortly afterward a number of new tumors arose and the lymphatics of the groin began to swell. These new growths as well as the lymphatics were removed, and parts of them brought to the laboratory. Case 4th. A woman of unknown age was operated on for cancer of the breast in the German Hospital of New York City in 1875. A few of her indurated lymph glands were taken from the axilla, and since then had remained in Dr. H.'s possession.

The last specimen on the study of which this paper is based, I removed, by a partial Lisfranc amputation, from the foot of a lady living in the country, about four miles south of Parksville, Ky. The microscope showed it to be a rapidly growing carcinoma.

The first three specimens showed all the stages of invasion, but the fourth contained nothing but the fully formed cancer tissue.

The transmission of cancer from a primary focus to the adjacent lymph glands is probably done by a transportation of the epithelia of the cancer through the lymph channels to the ganglion. This we know is sometimes done, for in case No. 1 we saw a few epithelia scattered among the lymph corpuscles of the cortical substance, their size and shape easily distinguishing them from all their surroundings.

Of course this will not justify one in denying that the fluid portion of the lymph coming from cancer, the so-called cancer juice, does not transmit the infection. We are sure, however, that cancer epithelia are carried and lodged in the lymphatic glands, but I am equally certain that we cannot yet explain why they or the juice can transform the structure of a normal tissue into that of cancer.

We have a great deal to learn yet before we can understand what gives the power of infection to the elements of cancer and sarcoma.

As I have already intimated, in the first three specimens I could trace the changes leading to the formation of cancer tissue. The first stage that we found was in that part of the gland where no natural fibrous trabeculæ separated the healthy from the diseased tissue. This consisted in a gradual melting down or running together of the lymph corpuscles, and thus forming large multi-nuclear protoplasmic masses, the so-called myeloplaxes. I have never seen these formations in a healthy adenoid tissue; once, however, I did see a few small ones in a hypertrophied tonsil.

There is no doubt in my mind that they spring from the confluence of the lymph corpuscles in all their different stages of development, as well as from the mucous threads that are their matrix.

This is not the first time that such a confluence of protoplasmic bodies has been proved to occur, for some time ago Ziegler sealed two small pieces of glass partly together and put them under the skins of rabbits. Shortly afterward he found that they contained these myeloplaxes, the only possible source of which was the migratory corpuscles.

In the lymph follicle the corpuscles are connected to each other by delicate offshoots of living matter, which pierce the separating layer of liquid, so that we can easily understand that all that is necessary to the formation of a myeloplax is the fusion of their jelly-like inter-trabecular substance.

Within one of these large lumps of protoplasm a number of nuclei fade and are transformed into a uniform reticulum of living matter, and thus a formation originates which resembles the myeloplaxes that are seen wherever bone is about to be made, or wherever it is reduced in the processes of growth and of inflammation.

Where bone is forming they mean that the territories of bone tissue are first laid down in the shape of a myeloplax, but where it is being reduced they mean that the territories, by the liquefaction of their basis substance, are brought back to their original protoplasmic state.

As I have already said, the process of confluence of formerly separated corpuscles is splendidly shown in the earliest stages of a growing cancer. In the invasion of a lymph gland its central portion is generally first involved. Frequently we found an inter-follicular string completely transformed into a continuous protoplasmic mass, but still retaining its original shape. These masses of protoplasm are supplied at regular intervals with large globular or oblong nuclei, which it is highly probable are newly formed and have very little to do with the original nuclei of the lymph corpuscles. It has been shown that the myxomatous reticulum holds a delicate network of living matter, which after the liquefaction of the basis substance held in its meshes, can reproduce protoplasm. This is most probably the process through which the fibrous framework is merged into the same mass with the corpuscles. These masses are coarsely granular, which means that they are freely supplied with living matter at the points of intersection of its network.

The next stage ensues through the appearance of the cement substance in the shape of straight, light lines arising first in the midst of the protoplasm between the nuclei. Under the microscope we see but one projection of the cement substance, but, in fact, it must be considered as a cloak enclosing polyhedral bodies, which are the epithelia. At first the cement lines are scarce, and in many places traversed by broad bridges of protoplasm. Later on it assumes a regular polyhedral shape, though it is always pierced by delicate spokes of living matter, which are the

inosculations of the reticula of living matter contained by the neighboring epithelia. These threads are the prickles of Max Schultze. The next feature is the formation of a frame of connective tissue, which divides the large protoplasmic mass into small alveoli, the cancer nests. The first trace of this formation is the appearance of delicate nucleated spindles, which by being split up into very minute, slender spindles, build up the fibrous basis substance. Its ramifying blood-vessels are formed at the same time.

Not infrequently the cancer nests in the midst of lymph glands exhibit concentric onion-like layers of epithelia, which in all probability are the result of pressure from the contraction of the surrounding connective tissue. In the centre of a nest we often see epithelia undergoing fatty degeneration. Sometimes it has gone to such an extent that a fat-plug is produced, the so-called cancer pearl, which is surrounded by flattened out horny epithelia. I found this concentric arrangement in the first three cases, but the fourth exhibited a fibrous frame enclosing irregular alveoli filled with large granular epithelia, but without any regularity of position. This is generally known as medullary cancer, the other as epithelioma. The essential points in this study of the lymph glands are that their invasion by cancer shows itself first by the melting together of their components, and by this means forming large protoplasmic masses. These, in turn, by the formation of the cement substance, split up into polyhedral epithelia, which in groups become ensheathed by vascularized connective tissue, and thus give rise to the cancer nests.

The study of the fifth case, which I have already said was that of a rapidly growing primary cancer of the foot, drove me to the conclusion that almost exactly the same state of things is going on in cancer wherever it is found. For along the edge of the fully formed carcinomatous tissue we

saw other tissues that showed all the changes that I have just described. I was also convinced by this study that the infiltration of round corpuscles that is always found surrounding a carcinoma is really a part of the metamorphosis, and not an inflammation caused by the irritation of the growth, as has been taught by some. They are exactly the same thing histologically as the lymph corpuscle, and it is by their fusion that the myeloplaxes are formed. Thus, I think we are warranted in saying that they are but a step of the fixed tissue corpuscles in their retrograde metamorphosis to the foetal tissue from which the fully grown cancer nests spring.

For my clinical reader I have but one deduction from all this histological work, and that is, in his operations on carcinoma, to make a clean sweep of all tissues that seem infiltrated in the slightest degree. For if he removes all the completed cancer tissue and leaves only a small portion of this infiltration, he has left the most dangerous part of the whole growth, that which is still progressing.

AN EXPERIMENT IN CARDIO-SPHYGMOGRAPHY ;
IN WHICH THE HEART, CAROTID, FEMORAL,
RADIAL, AND POSTERIOR TIBIAL ARTERIES,
AND THE CORRESPONDING TIME, WERE TRACED
SIMULTANEOUSLY ; WITH REPRODUCTION OF
THE INSCRIPTIONS OBTAINED.

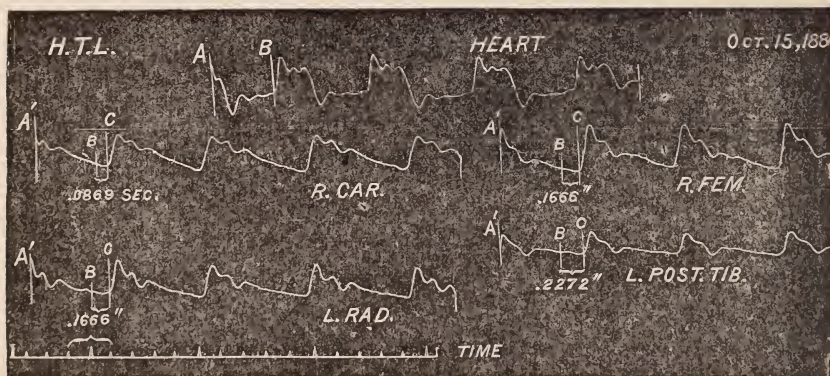
By A. T. KEYT, M.D.,
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THE accompanying illustration is a *fac-simile* of the surface of a glass slide on which were traced the pulsations of the heart and the corresponding pulsations of the carotid, femoral, radial, and posterior tibial arteries, with the time in fifths of seconds. The traces were taken simultaneously by means of five transmission sphygmographs and a chronograph arranged to write upon the same surface. The sphygmographs were sensitive and true and in every respect uniform with each other, and the chronograph was tested for correct time. The subject was a healthy man aged twenty-eight years, placed on his left side to facilitate the taking of the cardiac trace. The pulse-bases were held in their respective positions by the operator and requisite number of skilled assistants. In this manner the experiment whose results are here reproduced, and many similar ones, were made.

In further explanation: The lines A, A', were made by the writing levers with the slide at rest, and are therefore

synchronous signals showing the exact point of evolution of each pulsation at the instant indicated. The lines B and C are artificial, but drawn with care. The first, B, is parallel to A, and cuts the basal or beginning point of the cardiac pulsation; the second, C, is parallel to A', and cuts the basal or beginning point of the arterial pulsation. The space B-C on each pulse line shows the difference in distance between AB and A'C, and, measured on the chronogram, expresses the delay of each pulse on the heart.

Manifestly the distinctive feature of this species of representation is that the same pulse-wave in its transit along



the arterial ways is written out as it passes the different stations, and in association with the cycle of the heart which sends it forth—the heart and pulse at the several arterial points being traced simultaneously. The method by the duplex instrument,¹ in which the movements are traced successively, two by two, throughout the series, gives practically the same results; but inasmuch as the pre-sphygmic interval and the pulse-wave velocity both vary, evidently, the highest precision in these representations is alone attainable by means of the multiplex instrument.

¹See *N. Y. Medical Journal*, July, 1877, and February, 1878.

And not only does the method insure this precision, but the multiplicity of the facts it gathers is also remarkable. One experiment by it well made on an average healthy man is sufficient to solve a large part of the problems pertaining to the form, chronometry, and relations of the normal cardiac and arterial movements. Thus, in comparison, while nine separate experiments are required by the duplex method to determine with nearest permissible accuracy the time-relations between the heart and arterial points named, and between the arterial points themselves, one single experiment by the multiplex method shows all these with absolute precision.

Nevertheless, it is indeed fortunate that the simpler combination fulfils all clinical, and much the greater part of physiological, purposes; for this is easily managed, and with practice can be successfully applied by the operator alone, while it is also happy that for exceptional physiological researches the transmission sphygmograph admits of combination in any required number, and the apparatus so formed can be successfully employed with the aid of skilled assistants.

Among the many demonstrations of the experiment in question we here instance the following concerning the pulse successions :

The cardio-carotid time-difference is	.0869	second
The cardio-femoral “ “ “	.1666	“
The cardio-radial “ “ “	.1666	“
The cardio-posterior tibial “ “	.2272	“
The carotid-femoral “ “	.0797	“
The carotid-radial “ “	.0797	“
The carotid-posterior tibial “ “	.1403	“
The femoral-posterior tibial “ “	.0606	“
The radial-posterior tibial “ “	.0606	“
The femoral-radial time-difference in this case is <i>nil</i> .		

Having these figures it only remains to know the arterial

distances between the points named, to be able to compute the velocities of the pulse-wave over the different arterial lines. These distances by external measurements and estimates are the following:

Cardio-carotid	distance,	7 inches.
Carotid-femoral	"	18 "
Carotid-radial	"	23 "
Femoral-posterior tibial	"	33 "
Carotid-posterior tibial	"	51 "
Femoral-radial	"	5 "

Therefore, the velocity of the pulse-wave

between the carotid and femoral is 226 in. per sec.

Between the femoral and posterior tibial, 544 " "

" " carotid and radial 288 " "

" " carotid and posterior tibial, 363 " "

And hence the corollary:

The velocity of the pulse-wave is slowest along the aorta, fastest along the arteries of the lower extremities, and intermediate along the arteries of the upper extremities.

Again the data at hand permit us to approximately determine the duration of the ventricular pre-sphygmie interval. The velocity of the pulse-wave between the heart and carotid must be very nearly the same as that between the carotid and femoral. Accordingly, on the basis of latter and seven inches distance, we arrive at $\frac{7}{226} = .0309$ second as the time required for the pulse-wave to travel from the aortic orifice to the carotid point. This value deducted from the full cardio-carotid time-difference, namely, .0869 second, as recorded, gives .0560 second as the pre-sphygmie interval, or time comprised between the beginning of ventricular contraction and the opening of the aortic valves.

The experiments on animals and on the schema, notably those of Marey, having for object the elucidation of the

form and successions of the pulse-wave sent by the heart along the arteries, are thus supplemented by the experiment on living man himself, in which the pulsation of the heart and resulting arterial pulses are truly recorded in form and time, and their actual relationships to each other.

PERIOSTEAL PRESERVATION IN AMPUTATIONS OF THE LEG.*

By JOSEPH D. BRYANT, M. D.

NEW YORK.

THE title of this paper is not suggestive of any new ideas, nor will it appear as the reading progresses, that any thing of a revolutionary interest has fallen under the observation of its author. It is not even deserving of the designation, "old wine in new bottles." The only reason, hackneyed though it be, which I have to offer for having selected this theme, is the desire on my own part to elicit a discussion on the practical worth of periosteal flaps, as they are often called, on the part of those who have had a more extended observation of their utility than myself, at the same time to bring forward, incidentally, such practical proof as may have fallen beneath my own notice, bearing upon the manner of making them, their subsequent usefulness, complications, etc., etc. The early history of periosteal preservation, an expression which will be in this instance used synonymously with "periosteal flap," will form but a small portion of this paper. The influence of the periosteum in the reproduction of new bone in the presence of its diseased prototype, is a fact which has created more or less attention since, and even before the days of John Bell, who was familiar with this peculiarity of the membrane. It is not necessary, however, to quote from the

*Read before the New York Academy of Medicine, on April 20, 1882.

experience and observation of John Bell, or any surgeon-author, or practitioner of other than our own day and generation, to prove the power possessed by this membrane to reproduce the type of the bone which it normally envelops, protects, and nourishes. The labors of Drs. James R. Wood, Sayre, Markoe, Stephen Smith, Hamilton, and many others in this field, the results of which can be found in the private and public collections of this and other cities, speak loudly in commendation of the judgment and skill which have been shown, and establish positively the reproductive power of the periosteum when associated with diseased bone. The phase of the subject to which I desire especially to call your attention this evening, relates to the ability of healthy periosteum, when separated from healthy bone, to produce bone of a practically normal texture. Much time and not a small amount of labor have been expended upon this branch of the subject, notwithstanding which, as yet, it has not been sufficiently proved by practical observation, to be entitled to be considered an established fact of enough importance to become a part of the woof of surgical teachings and text-books. Many authors allude to it only; others speak of it in a cursory manner, which plainly indicates that they have little confidence and less experience in the matter.

In order to proceed understandingly, let this branch of the subject be divided into several questions. It will serve the important purpose of simplifying and shortening this paper, and at the same time bring the points for discussion directly before you.

First, can healthy periosteum be separated from the healthy bone of the human subject, and its integrity be sufficiently preserved for it to perform its characteristic function—the production of bone? If a belief could be based upon the observations which have been made on the lower

order of animals, this question could, without any hesitation, be answered in the affirmative. The experiments of Ollier on rabbits conclusively proved, that in this animal this membrane, when displaced by transplantation or otherwise, still retained the power of producing bone.

In the human subject these experiments have proved less satisfactory, having been much more uncertain in their results.

That bone has been found associated with the detached periosteum of the human subject is unquestionable, yet, whether it was removed from the bone by the same force which caused the detachment of the periosteum, or whether it developed subsequently from it, has, in many instances, not been satisfactorily determined. I mean when I say satisfactorily determined, determined with that degree of assurance which carries a positive conviction of its certainty. If this question be considered from an anatomical standpoint, and the periosteum be divided into two layers, the external or fibro-vascular, and the internal or cellular layer, often called, according to the fancies of the various authors, "osteo-genetic," "osteal cell layer," "subperiosteal blastema of Ollier," etc., then the answer must depend upon which of these two layers is essential to the reproduction of bone. If the presence of the fibro-vascular layer be only necessary for the reproduction, then success is assured in the beginning, since it can be easily elevated in nearly every instance, ordinary caution only being sufficient to maintain the integrity of its vascular supply. It is different, however, regarding the inner or cellular layer; it, being of a blastemic nature, clings closely to the outer layer and to the bone surface upon which it rests. It does not seem possible that this subperiosteal blastema, which is of a plastic consistency, can be entirely removed from the bone surface, and it is a fact, that microscopical observations have proved it

to be impossible; still, with some portion of the cell layer remaining upon the bone, and the absence of detached bone nuclei on the inner surface of the separated membrane, this membrane has been observed to produce bone. If a goodly portion of the blastemic layer be removed with the membrane, it is probable that bone will be produced, for, as is well known, while the periosteum is the most important, all tissues in contact with bone aid in its development and reproduction, and no one, as yet, has been able to positively assign to each an independent action in what must therefore be considered a common task. The fibro-vascular and cellular layers have, undoubtedly, a co-equal and inter-dependent importance; the chief function of the former being to supply the elements from which the latter elaborates the definite structure. The age of the patient has been found to exert a marked influence on the production of bone from detached periosteum. In early life when the bone growth is rapid, the vascularity of the outer layer is much greater, and the cellular layer is much thicker, than in later life. In adult life, when the dimensions of the bones become established, and repair has but to keep pace with normal disintegration, then the cellular layer is lessened in thickness, the vascularity of the outer layer diminished, and the periosteum becomes more firmly attached to the bone.

Finally, in old age the cellular layer may disappear, then the fibro-vascular layer clings closely to the bone, its function being principally that of protection. It is at this period only, that any great difficulty is experienced in separating the membrane from the bone it surrounds. Ollier in 1864 asserted that one of the conditions especially necessary to success, was a firm, thick, and well-vascularized periosteum. This condition is characteristic of the periosteum of youth, likewise of that which is subjected to the

stimulus of diseased bone ; hence the reason why the periosteum of youth and of diseased bone so readily performs the functions of reproduction. The experiments of Langenbeck, Lücke, Stokes, and Ollier have shown conclusively that the membrane which we call the periosteum can be detached from healthy bone, and, after such detachment, bone will be developed from its inner surface of sufficient amount to be entitled to special consideration as an element of usefulness in operative surgery. The success attained is in direct ratio to the age of the patient and the caution observed in the separation of the membrane. In fact, all the tissues in contact with the bone must be treated with the respect commensurate with the importance of the trust to be reposed in them. In view of these facts, this question can be safely answered in the affirmative.

A second and multiple question can now be asked. Is this secondary growth of bone constant in occurrence, durable in existence, and useful to the patient? The experiments of those already mentioned show it to be not constant in its occurrence. The reasons for this are not quite so clear as those showing why it should occur. The rule is, however, that in properly selected cases production of bone does take place ; its non-appearance often depending upon the inscrutable reasons which modify or prevent normal actions in other tissues of the body. It is unquestionably true that the active vital processes characteristic of the periosteum of early and middle life exert a most powerful influence in carrying on this function. It is likewise true, that a careful removal of the periosteum, thereby maintaining the integrity of its vascular supply and the attachment of its cellular portion, can not be underestimated. In brief, the injudicious selection of cases, the useless and unnecessary bruising and crushing attendant upon the "poking-up" process, so often witnessed, may de-

stroy its vitality, or so modify its functions, as to bring the operation into disrepute. If proper precautions be observed in the selection of cases and manipulation of the tissues, new bone will be produced within four or five months, which is not, however, *in all instances permanent*. Billroth, in 1868, pointed to the liability of the absorption of the new bone tissue, especially when the subjects were advanced in life or extensive suppuration had occurred. This fact applies equally well to new bone attendant upon necrosis, or developed from the detached periosteum of healthy bone. In young subjects, and where union occurred with little suppuration, he admitted the value of the method. It is proper to notice at this time, that when the new bone was absorbed, the fibrous portion of the periosteal tissue still remained to protect the osseous end in the stump.

In bone forms, is it useful to the patient? This portion of the question can be best answered, by recounting the advantages assumed to be gained by the use of periosteal flaps. They are said to prevent necrosis and atrophy of the end of the divided bone; to prevent the entrance of discharges into the canals; to aid in preventing retraction of the flaps; to prevent the adhesion of the cicatrix to the extremity of the bone; and to provide by the bony growth of a firm extremity, which obviates the danger of a tender and irritable stump. If one half these aims be attainable, the patient must then be inestimably benefited. The influence exerted in preventing necrosis is of an indirect rather than a direct nature. The great care necessary to be used in dividing the bone close to the periosteal reflection, without injuring the reflected membrane, is one of the most important of the preventive influences against necrosis. It is no doubt true, that if undue separation takes place, the displaced membrane will, when replaced in contact with the bone, exert a protective influence and may become reunited to it.

Atrophy and a consequent conicity of the end of the bone is prevented by the maintenance of the nutrition of its divided extremity, through the attachments formed between it and the periosteum reflected over it. The sheltering influence afforded to the end of the divided bone by the periosteal hood, protects it from the discharges arising from the soft parts, thereby being beneficial as a preventive of osteo-myelitis, caries, and purulent absorption. The ability to aid in the prevention of the retractions of the flaps no doubt exists, but in such a slight degree as to be of little practical importance. The power to prevent the adhesion of the cicatrix to the end of the bone is, in my judgment, well founded, and is of itself alone of sufficient importance to merit the closest attention. It may be said that, if sufficient care be exercised in making flaps of proper form and length, and the necessary precautions be taken in their subsequent dressing and care, this very objectionable feature of many stumps will be reduced to a minimum. This assertion, however, does not form a conclusive reason against it as a preventive measure, but rather serves to emphasize the necessity of using every means possible of obviating the occurrence of such an unfavorable result. It is said, that if in the near future, bony spiculæ are to shoot from the flap into the soft parts, to irritate the stump and torment the patient, requiring an operation for their removal, the benefit is gained at too great cost to the patient and the reputation of the surgeon. There are those, however, who, with a full knowledge of the liability to the spicular growths, consider that the advantage gained by securing a movable cicatrix, even at the cost of a second operation, is too great to be relinquished. On examining into the question of adherent versus movable cicatrices, I found more practical knowledge could be gained by consulting with those mechanics who see more of the sequels

of amputations, as revealed by the character of stumps, than the busiest surgeon. I refer to artificial limb-makers, or, as they are more recently called, the producers of "compensative appliances." They soon learn by observation alone, that a stump with an adherent cicatrix is not only a source of annoyance to the patient, but often brings their most persistent and best-directed mechanical efforts to an untimely end, often subjecting themselves to no little vituperation and pecuniary loss.

Now, that something has been said in favor of the plan, your attention will be directed to certain objections that have been urged against it. The unqualified objection has been made that it is impossible to raise healthy periosteum from healthy bone, therefore; the plan must be a delusion and a snare, of no use to the patient, and an annoyance to the surgeon! It is hardly necessary, I think, to again state the reasons on which the first question was answered affirmatively; for, "if healthy periosteum *can* be raised from healthy bone, and its integrity be sufficiently preserved to reproduce bone," this objection can be urged by those only, who have based their conclusions upon improperly selected cases, or upon their belief of the impossibility of raising the periosteum in its reproductive entirety,—that is, of separating the cellular layer from the bone.

A tyro in surgery can easily raise the fibro-vascular layer from the larger bones, together with such portion of the cellular layer as may cling to it; provided, the subject be not too aged. If it be impossible to raise the periosteum in its reproductive entirety, this assertion against the futility of the attempt holds good only in so far as it bears upon the advantages claimed for the new bony growth; for the membrane, as will be shown, will still protect the bony canals from the discharges, nourish the bone end, and prevent an adherent cicatrix. The second objection, and

seemingly a pertinent one has been made, that at some time, more or less remote, small bony spiculæ will protude into the extremity from the detached periosteum, cause much annoyance, and finally require operative interference for relief.

One of the strongest exponents of the method, in reply to a question of mine bearing upon the liability to this contingency, said: "In but one or two of those cases which I have been able to follow for four or five years, have the bony spiculæ occurred, and in each instance have required surgical interference for their removal; yet" he added, "I consider the advantages gained in these cases to be a sufficient recompense to the patients for the additional trouble." Assuming that all the advantages claimed for the flap are to be realized, still, this objection has much force with those who have a natural timidity of assuring their patients of the exact state of things, which of course must be done in self-defence, if for no other reason; for, all secondary interference will, unless the liability be explained at the time of the original operation, create distrust in the minds of patients, and a disdain for the surgeon on the part of those who have secured serviceable stumps minus the periosteal flap. If its assumed advantages and subsequent complication be explained to the patient, he is quite likely to elect to take his chances without the periosteal flap, rather than to lay a foundation for a heritage of pins and needles, requiring after treatment for relief. About two years ago I had occasion to re-amputate the leg of an army officer, 40 years of age, at the lower third, on account of an adherent cicatrix which caused so much irritation as to render the limb useless for the attachment of an artificial appliance. The question of a periosteal flap was placed before him.

Its advantages and objections were considered, and he decided to let me do as I saw fit. It occurred to me that if

the bony spiculæ protruded into the end of the stump, that it must be due, assuming the cellular layer to be the bone-producing layer, to the periosteum becoming turned outward, or in some manner presenting its bone-producing surface toward the flaps covering the extremity, instead of remaining apposed to the divided bone surface. I had, moreover, on various occasions witnessed the elevation of the periosteum, or, rather its being pushed upward on all surfaces of the bone into a wad rather than a flap, and concluded that the crushing and mixing of its surfaces, due to the illogical force, to be the active cause of the thorny sequel. Often, however, greater care was taken, and the membrane was removed in conjunction with its super-imposed tissues. In the case in question a periosteal flap was determined upon, but, instead of removing the membrane from all the surfaces of the tibia, it was taken from its broad subcutaneous surface. A moment's examination will show that if it be removed from this surface it will possess ample width to cover the divided extremity of the bone. (See fig. 1.)

No difficulty was experienced in removing it along with the super-imposed soft parts forming the flap. It was only necessary to dissect up the soft parts for about an inch, then divide the periosteum transversely, and along the borders of the tibia bounding its subcutaneous surface, when with the handle of a scalpel the membrane was readily pushed upward in conjunction with the soft parts resting upon it. The remaining portions of the flap were dissected up as if no periosteal preservation had been contemplated. In this instance the so-called hood-flap was made, and of course united antero-posteriorly. This did not entirely please me, for it will be readily seen (see fig. 1) that with such a flap united antero-posteriorly, the union will prevent the equal application of the periosteal lining to the divided

end of the bone. Its inner portion will be quite well apposed to the bone, while its outer portion will assume a more or less vertical position; therefore, the flap, as a whole, will not be in a proper position, consequently interfering with union, increasing the amount of suppuration, thereby lessening its bone-producing power, and predisposing to eversion and possible bony spiculæ. Any form of flap united antero-posteriorly will, owing to the difference in the direction between the line of union and the direction of the subcutaneous surface of the bone from which the periosteum is raised, cause more or less tilting of that tissue. (See fig. 1.) If the membrane be separated independently of the flap this will be obviated, but at the possible expense of the integrity of its circulation and a greater danger of displacement and eversion. It is true this periosteal flap might be stitched over the end of the bone, but, to do this is to interfere with the freedom of the discharges occurring between the bone and periosteal membrane. This case made a rapid and satisfactory recovery, and the patient has had up to the present time a most serviceable stump.

The second case was an amputation at the lower third of the leg of a patient 50 years of age, for malignant disease of the foot. In this case the circular flap was made, instead of the hood, as in the former. The periosteum was raised, in conjunction with the soft parts, from the subcutaneous surface of the tibia, precisely as in the preceding case; but the co-aptation of the flaps was made obliquely, that is, in a line parallel with the subcutaneous surface of the tibia (see fig. 1), and, therefore, in a line parallel with the line of attachment of the periosteal flap to the bone from which it had been raised. This deviation allowed the upper portion of the flap, with its periosteal lining, to fall by its own weight directly and evenly over the divided end of the bone. (See fig. 2.) This oblique method of co-aptation, not only served

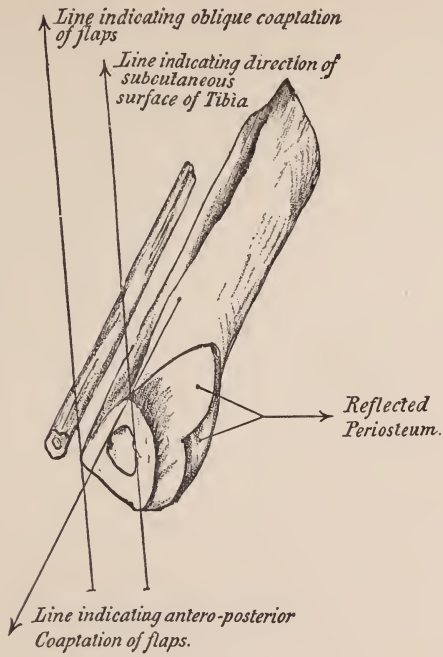


FIG. 1.



FIG. 2.

to properly adjust the periosteum over the end of the bone, but brought the line of union between the ends of the two bones; thereby doubly increasing the importance of the oblique co-aptation. The general principles applicable to the after-treatment of amputations were employed in each case. For obvious reasons, great care was taken to not allow the drainage tube to pass between the periosteum and the end of the bone to which it was apposed. The recovery from the operation was rapid, complete, and not attended by much suppuration. The patient was able to wear an artificial limb five weeks after the amputation, without any discomfort referable to the extremity of the stump. The cicatrix was perfectly movable, and pressure could be borne directly upon the end of the stump without causing any annoyance. Unfortunately, however, this stump too soon became typical of those "blessings that brighten as they take their flight," for, four months after the first operation, it became necessary to re-amputate at the knee joint for a return of the malignant disease at the middle third of the leg. It is unnecessary for me to say to you that the specimen was cherished with due care, and is now presented for your examination. It has been carefully examined by Prof. W. H. Welch and myself, with a view of ascertaining the following facts:

1st. The relation of the cicatrix to the end of the bones.

2d. The relation of the periosteum to the end of the tibia, and to the soft parts.

3d. If bone had been produced, and if so, its relation to the surrounding parts.

4th. The condition of the end of the tibia.

5th. The condition of the end of the fibula, which had not been covered by periosteum.

On longitudinal section, the line of the cicatrix was

scarcely discernible in the soft parts. (See fig. 3.) It was freely movable, and the integument was separated by a cushion of fat from the periosteum covering the end of the tibia. The periosteum was found to be firmly and evenly attached to the end of the tibia, sealing its extremity thoroughly. It could be detached from the end of the bone with but little difficulty, its continuity with the periosteum above was plainly to be seen, and its relation to the superimposed soft parts was unchanged, being similar in all re-

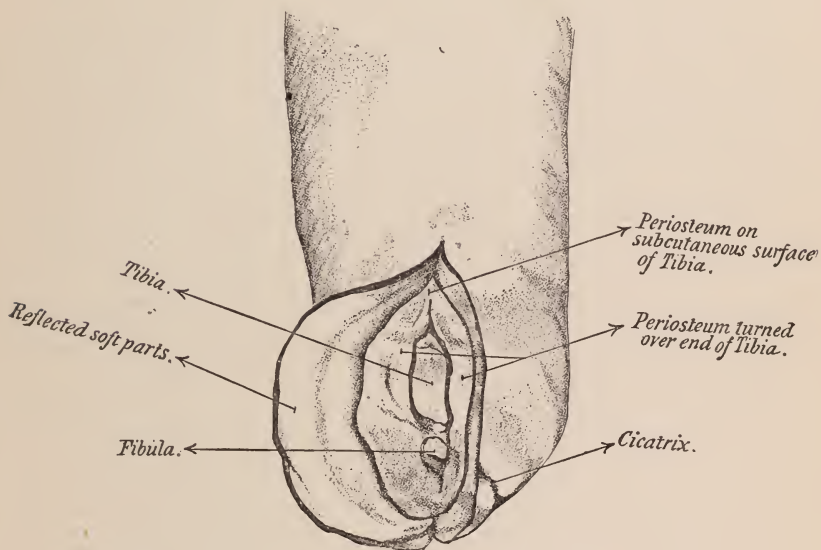


FIG. 3.

spects to that found above. There was not tangible evidence of the production of bone, although the attached surface of the membrane to the end of the tibia was rough and had minute particles of bone connected with it. These were considered to have been detached from the bone during the removal of the periosteum. The end of the tibia was but slightly rounded, due to very limited absorption of its borders, and was closely covered by the membrane. The small dark points showing the extremities of the Haversian canals

were numerous and distinctly marked, the whole thickness of the bone appearing to be composed of moderately compact bony tissue. The relations between it and the membrane were intimate, simulating in all respects those existing between bone and its periosteum elsewhere. The end of the fibula was covered with a fibrous tissue apparently continuous with its structure and that of the surrounding tissues. This specimen, in my opinion, teaches the advisability of periosteal preservation, and practically substantiates the reasons which have been advocated in its favor.

If stumps can be made which present the features of this one, it is a matter of little importance whether bone be produced or not. I am well aware that one opportunity to examine a specimen of this nature cannot be sufficient to establish a rule of faith or practice. These opportunities are, however, exceedingly rare, and I think their results should be made known at once, in order that the good that may arise shall increase their number, or that the evil coming therefrom shall check the desire that gives them birth.

A CASE OF TUBERCULAR LEPROSY ORIGINATING IN CONTAGION.*

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THE history of the following case of tubercular leprosy is reported to this Association, rather on account of its important etiological bearings, than in consequence of any clinical features of special interest.

Mrs. B.; 40 years of age, applied for relief at the out-patient department of the University Hospital, October 18, 1880. She was of medium size, had blue eyes and light hair, and gave the following history: Her parents came from Hanover, where they were born, many years ago. The father died of "consumption," the mother of "change of life." She has two brothers and two sisters, in good health. Several brothers and sisters died in infancy, of diseases the nature of which she does not recollect. One brother died at 47 years, of dropsy. The patient was born and raised in Baltimore. In her sixteenth year she married and went to the Eastern Shore of Maryland to live. Her husband kept store in Salisbury, Worcester County. They returned to Baltimore after several years, and remained until the beginning of the late war, when they again moved to Salisbury. Nine years ago they again came to Baltimore, where they have resided ever since. It will thus be observed that Mrs. B. has never been beyond the limits of the State of Maryland.

She has had nine children, but no miscarriages. Five children still live. Those who died were carried off by disorders incident

* Read at the 5th annual meeting of the American Dermatological Association at Newport, August 31, 1882.

to infancy or childhood. While living at Salisbury, she ate much salt meat, and but little fish. She has suffered from malaria, but not during the past nine years, though living in a malarious locality. Her condition of life has been fairly well-to-do, and her general health pretty good until five years ago, when she began to feel unwell. She was nervous, often had sick headache, pain in the back, and was often feverish. It was not, however, until May, 1878, that she noticed any cutaneous disorder. At this time she observed, while pregnant, yellow spots upon her thighs. Three weeks after the birth of her infant, similar spots were noticed upon the trunk, and in a short time were pretty generally distributed, and became of a much brighter reddish color.

At this time her hands and feet were swollen and tender. She could stand with difficulty. Yellow spots have been copiously present upon her trunk, head, and extremities ever since, though they are not so intensely colored as formerly. Her ill-health continued, but the spots remained the only eruption until the present year (1880), gradually changing to a deep salmon color. Last spring Mrs. B. noticed some lumps upon her face, and shortly afterward upon her arms. Previous to this, during the winter, a bleb as large as a hickory-nut appeared upon the fibular side of the right foot. It gave much pain, and healed slowly. This was the only bleb the patient had had.

Mrs. B. became pregnant toward the end of April, and since that time the nodules rapidly increased in numbers and extended to nearly every part of the body, remaining, however, scanty upon the trunk. Since the beginning of her pregnancy, she had felt wretchedly uncomfortable, and consulted quite a number of physicians, who, however, failed to afford more than temporary relief.

At the date of her visit, her condition was carefully noted. Over the neck, throat, breast, back, arms, feet, and hands (faintly upon the thighs and legs) were flat, circumscribed patches of irregular outline, without elevation and free from desquamation. They varied in size from that of a split pea to that of the palm, frequently becoming confluent. These spots were almost entirely pigmentary. Their color was of a dull, dirty, pinkish-brown hue or salmon color. Over the front part of the neck and face, this coloration was diffusely spread, and involved the entire surface. While these pigmented areas had no elevation, the skin involved in them was very evidently thickened; in some places, especially about the neck and shoulders, it was twice as thick as normal.

This thickening was sharply limited by the areas of pale brownish pigmentation. The portions of skin between these patches were of normal thickness. Upon the right cheek was a dull, dead-white spot as large as the nail of the little finger. This had made its appearance within the last two years, and closely resembled a scar. The finer natural lines of the skin of the face and neck were less distinct than normal, but the coarser lines and wrinkles were exaggerated.

The palms and soles were of a dull yellowish color, and had a glazed appearance. Indeed, the general surface had a muddy look, and, even where no spots were visible, the eye received a dim impression of mottling.

Mrs. B. asserted that these spots were not permanent, but that, after remaining a while, they slowly faded and disappeared, presently to be succeeded by new ones irregularly distributed. The feet and legs were very œdematous. The dorsal surfaces of the feet were puffed and cushion-like, and had a shiny appearance. The backs of the hands were similarly affected. The extremities had been in this condition for nearly two years, and locomotion had been very painful.

During the preceding spring, small nodules began to be discovered over her face as already noted, and rapidly increased in size and numbers. When first seen by me, they varied from pin-head size to pea size, and were scattered in very great numbers over the face, neck, arms, forearms, hands, thighs, and legs. Upon the trunk were but few, situated in the infra-clavicular region. Large numbers of these nodules projected from the skin, but multitudes (of smaller size) could be felt imbedded, though invisible, particularly in the subcutaneous tissue of the thighs. Those that were visible were paler than the surrounding integument or were darker, or, occasionally, of livid appearance. Their surface was smooth and shiny, and their structure was rather dense. They were often seated in the macules. Nodules could be felt deep under the skin of the soles.

There was considerable thickening of the eyebrows and lobes of the ears, but this seemed to depend rather upon diffused thickening than upon distinct nodules, although numbers of the latter were present in these localities. The characteristic facies of leprosy was distinctly foreshadowed.

Sensation had been decidedly modified since the beginning of the complaint. There had been no itching, but, rather, a pronounced numbness. This had never been fixed and permanent,

but parts that had once been benumbed, had recovered perfect sensibility after a while, new areas of numbness appearing from time to time. There had never been complete anæsthesia. The point of a pin could be felt wherever pressed into the skin, rather more painfully upon the unaltered portions than upon the macules. As tested by the æsthesiometer, however, diminished sensibility seemed rather to be encountered in certain areas, irrespective of the distribution of the macules.

A rhinoscopic and laryngoscopic examination kindly made for me by my friend, Dr. H. Clinton McSherry, revealed thickening and granulation of the mucous membrane of the posterior nares, and on the left side of the anterior nares. The epiglottis was seen to be slightly thickened, but the two arytenoid cartilages and the inter-arytenoid fold were very much thickened. Weakness of the eyes was complained of, but this was found to be due to conjunctival hyperæmia, her vision having been ascertained to be normal by my colleague, Prof. Chisolm.

Her functions were, for the most part, naturally performed. Heart and lungs were healthy. Urine was normal in quantity, and free from albumen and tube casts, but with a copious deposit of lime-oxalate octohedra. Her temperature was 99° F. (She was at the time of this examination about six months pregnant.)

As Mrs. B. approached the termination of her pregnancy, multitudes of small tubercles appeared over her face, neck, buccal and faucial mucous membrane, and extremities. She would have attacks of fever, which would keep her in bed for several days, and upon their subsidence a notable increase in the number of tubercles would be observed. The œdema of the legs markedly increased. Many tubercles became livid in color and, softening, broke down into small excavated ulcerations. In numbers of the tubercles, disintegration took place in a peculiar manner. Each would become surmounted with rather large vesico-pustules, the ulcer appearing after the rupture of these. The number of these tuberculo-pustules increased rapidly during the last weeks of her pregnancy, and the mucous membrane became invaded by them. The mouth (tongue, hard and soft palate, gums and inner surfaces of the cheeks) became the seat of great numbers of them, speedily passing into superficial ulceration.

At the date of her confinement there were distributed over her person several hundreds of these small ulcers that had resulted from tubercles. The eyebrows, lips, and ears, had now become de-

cidedly tuberos. The child was a healthy boy. After her confinement Mrs. B. improved rapidly. By February 28, many ulcerations had entirely healed, leaving scars. Very many small nodules had disappeared without suppuration, and many could be observed in process of involution, becoming smaller, losing their hyperæmia, and becoming deeply pigmented. Most of them disappeared in this latter manner, and the hand, passed along parts of the skin where they had been most abundant (inner surface of thigh, for example), could now perceive almost nothing abnormal. New nodules continued to appear, however; many of the old ones remained unchanged; the thickening of the skin and the maculations remained pretty much as before, and the face was gradually assuming a more characteristic expression.

The case up to the present time has been slowly progressive, shows now, August 15th, all of the above-mentioned symptoms in a somewhat exaggerated manner; the ulcerations, it is true, are nothing like as numerous as they were just previous to her confinement, but they have become numerous and intractable on the lower extremities, and occasion much distress. The fingers and toes show, as yet, no characteristic changes, but look somewhat clubbed. Both ulnar nerves can be felt, enlarged and thickened. The general condition remains about as usual. The last baby is being nursed at the breast and remains apparently healthy, as does also the one now about 2 years old, who was born subsequently to the appearance of the mother's leprosy.

I have given the preceding history at length, at the risk of proving tedious, not because any unusual or striking features became manifest, but in order to make it clear that I had to do with a perfectly well-marked case of tubercular leprosy, in view of the very important etiological aspects of the case. After careful interrogation I had failed to discover any causative influence whatever, and was beginning to conclude that it was a purely sporadic leprosy of undiscoverable origin, when one day Mrs. B. remarked that her neighbors were beginning to notice that her appearance was assuming a resemblance to that of a man named Brown who had lived in the immediate vicinity some years previously.

I soon ascertained the fact that this Brown was the same individual whose case was reported as one of tubercular leprosy, in the *Maryland Medical Journal* for July, 1878, by Dr. George H. Rohé,¹ and easily obtained an account of the movements of my patient since her return to Baltimore in 1870.

After living for one year in a neighboring street, she moved to No. 139 Ridgeley Street, where she lived two years. At the time of this removal, the man Brown lived at No. 113 of the same street, or thirteen doors further up. The families became intimate, and Mrs. B. was once or twice in the house of Brown, visiting his wife. During the next year, however, Brown removed to No. 141 Ridgeley Street, or next door to the house occupied by Mrs. B.'s family. Here Mrs. B. occasionally saw Brown, but never had any thing to do with him, never even shook hands with him, nor ate any thing in his house. He had been in her house, but had never eaten a meal there. After Brown had been living at No. 141 for one year, the B. family removed to their present residence, No. 102 in the same street, just opposite their old home. Brown moved out of the neighborhood four or five years ago. (These houses are small two-storied structures, of about 15 feet frontage, and, with a small back building, containing five or six small rooms. They are in immediate juxtaposition.)

At the time she first became acquainted with Brown, he had eruptions and nodules upon his face and was in bad health, and was said to have suffered from frequent attacks of "erysipelas."

Mrs. B.'s children appear to be perfectly healthy, and those born since she became leprous remain, as yet, without a trace of their mother's disease. Her husband is a healthy laboring man, with no evidence of disease.

¹The portrait of this man appears in Dr. George H. Fox's "Atlas of Portraits of Skin Diseases" as a representation of tubercular leprosy.

During the period of its greatest prevalence in Europe, in the eleventh, twelfth, thirteenth, and fourteenth centuries, leprosy was universally believed to be contagious; and at the present day, in those places where it is most frequently encountered, the popular belief in its contagiousness remains unshaken. Although the disappearance of the malady from the greater part of Europe has been attributed to the isolation enforced upon lepers during generations, rather than upon any especial change in climate, habits, diet, or in the general condition of the people, the views of authorities seem to have, during late years, tended toward a disbelief in the contagiousness of leprosy, and most of those who have devoted the greatest study to the subject attribute its origin to other causes. Thus, Danielssen and Boeck, Virchow, Hebra, and others have thrown discredit upon the theory of contagiousness. The profession generally, however, does not appear to have been convinced, and those who deny absolutely that leprosy may spread by contagion are few. A few writers, indeed, strenuously maintain that the disease is disseminated in no other way.

Well-observed cases have been reported in abundance, where the disease could be directly traced to contagion; cases where the histories have been followed up with so much skill and intelligence that one could hardly fail to be convinced of their genuineness, were it not that they have always occurred in countries where leprosy has been known to prevail, where other causative influences might have been brought to bear, and where all persons were exposed to similar influences depending upon peculiarities of climate, soil, diet, habits of life, etc., to which the spread of leprosy has been variously attributed. Science demands, before the contagiousness of leprosy can be admitted as proven, that the evidence must be produced in countries where the endemic influence does not prevail, where lepers introduced

from other parts shall have communicated the disease to persons with whom they may have been brought into contact. The difficulty of fulfilling such requirements are undoubtedly great, but it would seem that in this country opportunities may fairly be expected to occasionally occur, since leprosy, where it prevails, has, with one or two exceptions, undoubtedly been introduced from without; and since in many sections the disease has never been known. Says W. Boeck (Neumann: "Lehrbuch der Hautkrankheiten," 5 ed., 1880, p. 54): "The United States of North America must be looked to as the fittest place for the elucidation of this question, since leprosy does not prevail there, and since there are known no other influences that have been considered the causes of leprosy."

The case that I have given in detail in the preceding pages fulfils, I think, all the requirements of the question. So far as I have been able to ascertain, no cases have ever been reported, where leprosy has originated within the borders of the State of Maryland. It is true that Munro, whose series of papers in the *Edinburgh Medical Journal* on "the etiology and history of leprosy" are decidedly the ablest presentation of the question, from the standpoint of a contagionist, writes: "It has been stated to me that cases" [of leprosy] "occur in the Southern States of America, and such cases are seen among the blacks at Baltimore, but whether of blacks from the West Indies, or natives of the States, I have no information" (August, 1877, p. 145). He does not, however, give the sources of his information, and it is probable that it was derived from rather vague statements of not very competent observers, or the references would have been given. Certain it is, that in the sense implied, leprosy is not met in Baltimore, either among whites or blacks. That cases of leprosy may have, at one time or another, been present in Baltimore without having been re-

corded is quite possible, but the fact remains that the vast majority of medical men in the city and State have never even seen a case of leprosy; nor with the exception of the cases I am about to refer to, has leprosy been treated in the city hospitals for a great number of years. The only cases of the malady that I can find any reference to, were reported to the *Maryland Medical Journal*, vol. iii., p. 147, by Dr. George H. Rohé. These were three cases of tubercular leprosy.

One patient was a young man, a native of the West Indies (Barbadoes), of English parentage. He became leprous at the age of five years. His parents were both healthy. He came to Baltimore when 15 years old, and was admitted into the University Hospital about 1870, where he was under the observation of Prof. Tiffany, who communicated the notes of the case to Dr. Rohé in a letter. This man subsequently died at the age of about 22 years. A second case was that of a woman, born in Baltimore of parents who never became leprous. At the date of Dr. Rohé's observation she was 46 years old, and the mother of four children the oldest 29 years old, the youngest 8 years old, all in good health. In the spring of 1855 she went to New Orleans, where she remained until the occupation of the city by the Federal troops during the late war. Subsequently, desiring to be near her husband, who was in the Confederate service, she resided in different parts of the South. In 1865 she returned to Baltimore, where she had since resided. Symptoms of tubercular leprosy appeared four years after her return from the South, and subsequently to the birth of her youngest child. This patient died in 1878, shortly after Dr. Rohé's report. When last heard from by Dr. Rohé, the child remained healthy.

The third patient reported by Dr. Rohé (the first mentioned in his paper) was the one with whom my patient

came into contact, and I need no apology for quoting the case somewhat fully. This man, A. B., was born in New York City, and was 54 years old. He was by trade a bricklayer. His parents had been free from leprosy, and he had two sisters who remained healthy. "In 1855, he went to Cuba, working for a gas company in St. Iago for 9 years." His wife, to whom he was married 21 years ago, joined him in Cuba, with their two boys, five years later. "In 1864 he left St. Iago for Baltimore, and has since then been living here, working at his trade until last summer" (1877). He dated his first symptoms back two years, but from the account given by my patient, it is most probable that he had manifested symptoms of the disease earlier. For already in 1872, they were next-door neighbors, and at that time B. is said to have had spots on his face, and to have suffered from frequent attacks of "erysipelas."

It is not now claimed that these are the only cases of leprosy that have ever been known in Baltimore. Indeed, as I have already remarked, it is altogether likely that occasional cases have been seen there, but I have been unable to discover any notice of such. At all events, I think I have made it clear that leprosy is one of the very rarest of diseases in Baltimore and Maryland, and is practically unknown to the medical profession there.

I conclude from all the foregoing considerations, that it is in the highest degree improbable that the only case of leprosy that has been reported as having certainly originated within the State should have developed, without recognizable predisposing influences, a few years after having been next-door neighbor to one of the only three lepers that have been observed, so far as I have been able to ascertain, in a city of 400,000 inhabitants for a period of, at least, many years, without having in some manner derived it from him. It is hardly possible to imagine such a coincidence as the result of accident.

Especially important, in relation to the question of the contagiousness of leprosy, are the recent discoveries of Armauer Hansen (*Virchow's Archiv*, 79 Bd., 1880), Eklund (Stockholm, 1879), and Neisser (*Breslauer Artzlichen Zeitschr.*, Nos. 20 and 21). The detection, by these observers, under the guidance of Hansen, of the bacillus lepræ cannot fail to exert a decided influence upon the future history of leprosy. With a view to seek a confirmation of the results of these writers, the entire lobe of the right ear of my patient was removed with scissors, and submitted, for histological examination, to my friend, Dr. I. Bermann, who has reported his conclusions in the accompanying paper.

I cannot close this paper without referring to the national importance of settling this question of the etiology of leprosy. In no country in the world is definite information upon this subject so urgently demanded as in ours. With Chinese lepers pouring in upon our Pacific coast, with Norwegian lepers settling in the Northwest, with an increasing number of lepers in the State of Louisiana, we seem to be threatened at many points. Should the disease prove to be contagious, as from an unprejudiced consideration of my own patient I am convinced it is, there can be no subject more worthy of the earnest consideration of our sanitarians and legislators.

THE BACILLUS LEPRÆ.

By I. BERMANN, M.D.,

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IN May, 1881, my friend Dr. I. E. Atkinson asked me if I would like to confirm, by microscopical examination, his clinical diagnosis of leprosy on one of his patients who had consented to have a piece of skin excised for that purpose. Our object was to see whether the discovery made by Hansen, Klebs, Eklund, Neisser, etc., as to the existence of the bacillus lepræ, would be verified by us, and thereby prove beyond doubt the existence of a solitary case of leprosy in this city. The literature of this subject is, considering the rarity of the disease, pretty extensive, especially as regards the pathological anatomy, and the histological changes occurring during its progress. To those who wish to refer to this literature I may say that the last paper of Dr. H. D. Schmidt, of New Orleans, published in the *Chicago Medical Journal and Examiner*, April, 1882, gives all necessary points on this subject.

It is not the object of this paper to enter into details of the pathological anatomy of lepra, especially as Dr. Schmidt has lately given such an exhaustive and exact description of it (*vide* ARCHIVES OF MEDICINE, Dec., 1881), with which, on the whole, I can fully concur.

The first author who called attention to the fact that rod-like organisms can be found in leprous tissue, blood, pus, etc., and that they bear a direct relation to the disease,

was Hansen. After him, Klebs and others have published similar results from their investigations of leprosy. These papers have not attracted the attention they deserve, as we find no mention of them even in the newest editions of our hand-books on skin diseases.

The discoveries of Weigert, Koch, Ehrlich, and others, regarding the use of aniline colors in examinations for bacteria enabled Neisser to make use of their methods in his investigations on the pathogenesis of leprosy, and the results of his researches have established, beyond doubt, the existence of bacillus lepræ. Not only was he able to detect and demonstrate the bacillus in all his cases, but also to cultivate them in blood serum and other fluids (*vide Virchow's Archiv*, June, 1881).

Dr. Atkinson and I took the piece of leprous tissue, submitted to my examination, from the lobe of the ear of the now living patient. It was hardened partly in Müller's fluid, partly in alcohol.

The sections were made after my dry-cutting method, and transferred first to turpentine, and then to absolute alcohol, in which they were kept constantly. After employing the usual methods of staining, without satisfactory results, I finally got a glimpse of the bacillus in the specimens by following the procedure described by Neisser in the aforementioned paper, and that of Weigert in the May number of the same journal. Neisser recommends that the sections be treated first with liq. potassæ (1:12), which, according to him, shows the bacilli without staining; but better results are achieved by using gentiana, methyl violet, or fuchsine, a one-per-cent. solution of these coloring agents being employed. The sections are subsequently washed in acidulated alcohol. Neisser expressly stated, however, that bacillus lepræ stains with more difficulty than any other bacteria; he likewise recommends an acid eosin-

hämatoxylin solution as especially effective for the demonstration of the bacillus lepræ.

I have been able to verify Neisser's results by these methods, but have derived more satisfaction by following the plan recommended by Weigert, which is to transfer the section from distilled water into a one-per-cent. solution of aniline blue or purple (the aniline blue which I used comes from Vogler, Son, & Co., of Baltimore) for a few seconds, and then washing it in distilled water. It is then transferred to absolute alcohol, which takes a large part of the color out again. This alcohol must be changed several times. The section is next placed in oil of cloves. By putting the section alternately into alcohol and oil of cloves, the coloring matter is almost entirely removed from the cell protoplasm, so that only the bacilli contained in the section are stained bright blue or purple. It is finally mounted in Canada balsam or damar varnish, after having remained in oil of cloves for about twenty-four hours. Sections prepared in this manner, and examined with a good $\frac{1}{8}$ or $\frac{1}{10}$ objective system and condenser, will show the bacillus most perfectly. The superior advantages of Weigert's method became apparent immediately upon examination. The groups of large cells, which constitute the special feature of leprous tubercles, appear to have retained more of the staining fluid upon superficial examination. On submitting them to a higher magnifying power, it becomes apparent that the intensity of color is due to the presence of deeply pigmented rods within the cells, and not simply to the staining of nuclei. The size of these rods equals $\frac{1}{8}$ to $\frac{1}{3}$ the size of a red blood corpuscle in breadth, and $\frac{1}{2}$ to $\frac{1}{3}$ in length. Almost without exception they are found most perfectly within the large "lepra cells" (Virchow), and irregularly distributed through them. These cells are usually four to eight times the size of a white blood cor-

puscle, and are lying close together with but little interstitial tissue. If the specimen is stained double, with eosin and violet, there will also be found distributed among the larger cells mentioned a few cells resembling the Waldeyer's plasma cells, *which stain intensely red with eosin, do not take the violet staining, and never contain bacilli.* The larger cells contain frequently several nuclei. Very often I find very deeply stained large cells, which on close examination prove to be filled almost entirely with bacilli of different sizes.¹



Section from leprosy tissue showing rod-shaped bacteria—*Bacillus lepræ*.

¹ As a curious coincidence I should mention that in some of the lymphatic vessels I find networks of fibrinoid substance containing at the same time bacilli. The same observation can be made in the lymphatics in syphilitic tissue, and in tuberculous tissue.

These large cell conglomerations I always find immediately beneath the rete Malpighii, and, cannot discover among them any blood-vessels. On that account I should consider them as tubercles, with highly developed cells about to undergo a rapid disintegration for want of proper nourishment.

The *Annales de Dermatologie et de Syphiligraphie*, October 25, 1881, contain a paper by Cornil and Suchard on the same subject, with very excellent plates, so that it seems almost unnecessary to add a plate to this paper, except that the one drawn for me by Prof. W. K. Brooks, of the Johns Hopkins University, is a correct copy of one of my sections.

In conclusion I wish to call attention to the fact that only a very strict observation of the rules given will enable investigators to achieve satisfactory results, and that, even in spite of most careful manipulation, specimens are liable to fade within 24 hours of their preparation.

Since the above was written I have found that by staining the specimens first either in a 1% eosin or Bismarck solution, before staining with the aniline blue, still better results can be obtained.

In the April number of the *Chicago Medical Journal and Examiner*, Dr. H. D. Schmidt, of N. O., discusses the question whether the bacillus lepræ is a reality or a fiction. That he has not succeeded in finding them in his specimens I cannot doubt, since it was testified that they could not be seen in them by microscopists in Chicago. I am satisfied that some fault in his method is alone the cause of his non-success, and I should be very glad to stain some of his material if he will send it to me, and believe that I could convince him, in this way, of the unfictional character of the bacillus lepræ.

A SUCCESSFUL CASE OF LIGATION OF THE COMMON CAROTID ARTERY WITH A CARBOLIZED NERVE.

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THE re-introduction of the broad animal ligature into surgical practice, and its present popularity, are due to the brilliant surgery of Mr. Richard Barwell. Before ligatures were made antiseptic by being carbolized, various kinds of animal tissue had been experimented with, adopted, and later abandoned.

In 1814, Physick¹ used chamois skin. Hartshorn preferred parchment. Jamieson, of Baltimore, tied the carotid, iliac, femoral, and other smaller arteries, with flat buckskin strings. No secondary hemorrhage followed, and the ligatures (which were cut short and left in the wound) were never heard of. The wounds healed generally by first intention.

Astley Cooper used the ligature recommended by Physick, but condemned it later.

Travers, Lawrence, Cawardine, Porta, and others, in addition to the materials just mentioned, used ligatures made from intestine, nerves, tendons, and raw hide.² All of these

¹ Cooper's "Surgical Dictionary," p. 195.

² Prize Essay of the American Medical Association, on "Treatment of Aneurisms," B. Howard, M.D. Printed by Collins, Phila., 1870, p. 25.

fell early into disrepute—gave way to silk and metallic ligatures, and did not appear in surgical literature until within the last few years. Carbolized sheep-gut threads twisted hard, small, and round, were brought prominently forward as safe and serviceable animal ligatures more than a decade ago by Mr. Lister, and have grown in professional favor.

In 1880, Mr. Richard Barwell published his cases (up to that date) in his booklet, "On Aneurisms; especially of the Thorax and Root of the neck."¹ The success he achieved was so remarkable and gratifying, that I determined to use his ox-aorta tape ligatures in a case which had come under my care, and on September 21, 1880, I tied the right common carotid, and the right subclavan, for the relief of a large aneurism of the ascending segment of the aorta.² The relief was immediate and marked. The patient improved for several months, the aneurism decreasing in volume.

In April, 1882, she exposed herself to unusual fatigue in very inclement weather, and contracted a violent bronchitis, which confined her to bed for weeks. The aneurism enlarged again, but never resumed its former size, nor did the patient suffer from it as she had done before the operation. In August, 1881, she was attacked with a severe diarrhœa, from which she died in the latter part of that month. With the kind assistance of Drs. Converse, King, and Cramer, I made the autopsy. Both arteries were completely occluded, and there was no remnant of the ligature, which had left a more noticeable indentation upon the subclavian than the carotid.

The subclavian was occluded only for a distance of about one half an inch. The collateral circulation was unimpeded in the internal mammary, thyroid axis, and other branches of this artery. There were some objections to the ox-aorta

¹ Macmillan & Co., London, 1880.

² *American Journal of Medical Sciences*, January, 1881.

ligatures, as I had prepared them. The knots were too bulky, and it was difficult to obtain them.

In looking about for a smoother and stronger substance I determined to use nerve tissue. I concluded that it was probable this material had been used before, although I had not heard or read of such a procedure until seven months after my operation, when, in looking up the subject for this article, I found (as heretofore quoted) that threads had been used as ligatures, which were made of "tendons, nerves," etc. I regret that I cannot give the name of the first experimenter, or his case or cases.

My own operation was the following.¹

Ellen W., æt. 53, Ireland, married, domestic, several children. Admitted to Mt. Sinai Hospital May 2, 1881, for malignant disease involving the right antrum maxillare, speno-maxillary fossa, cavity of orbit, and the frontal, sphenoidal, and ethmoidal sinuses. The right eye had been completely destroyed. For several months she had had severe headaches. In June a sharp hemorrhage occurred into the mouth and nares while she was sleeping. She awoke in time to prevent suffocation. The pains and occasional hemorrhage continued until the 27th of September, when I tied the right common carotid below the omohyoid, with the freshly carbolized sciatic nerve of a calf. My object was to arrest the hemorrhages, retard the growth of the neoplasm, and relieve the severe pains due to its presence. The ligature was cut off and left in the wound, which healed by first intention. The pulsation never returned in the temporal of the right side. The pains on the affected side ceased, but the increase of blood pressure on the opposite side caused "a singing in her head," and a disturbed feeling which was painful. These symptoms gradually subsided. The deeper portions of the neoplasm began to break down and suppurate in two or three weeks, and continued until her death on April 25, 1882. She left my service on account of erysipelas, with which she was attacked, and I did not see her again until a few days before she died. Dr. Magnin, of Bellevue Hospital, and Dr. Healy, of Hart's Island, kindly took charge of the patient for me.

¹ Notes by kindness of Dr. Cramer, House Surgeon Mt. Sinai Hospital.

On April 26, 1882, I presented the artery before the New York Pathological Society, together with the carotids of a horse and a large grayhound,¹ which had been tied with nerve ligatures. The artery was completely occluded and its continuity unbroken. There was a depressed ring, scarcely appreciable, at the point where the ligature had constricted it. No result could have been more perfect than was accomplished by this ligature. It was as large as the median or ulnar nerve in the humeral region of a full-sized man, and was taken from the calf, and kept in five per cent. carbolic water for twenty-four hours before using.

The advocates of the tape-like, carbolized, animal ligature argue that it is safer than the smaller, harder, violin-string sheep-gut ligature (or any other thread), since: 1. It includes in its pressure a larger area of artery, and hence does not cut into the coats of the vessel, either by the force of the ligature when it is tied, or by the subsequent expansion and friction of the arterial walls against the narrow thread. 2. It does not divide any tunic of the artery, simply jamming and wrinkling it, bruising the intima, causing a slight inflammation and proliferation of the endothelia, which result in permanent occlusion.

Howard, in his Prize Essay says: "The force with which the ligature is applied strangulates the vasa vasorum, and thus compels the part included in the loop and immediately beyond it to die. Through this thin remaining coat the ligature rapidly ulcerates. Roux, Hodgson, Brodie, Thompson, Erichsen, Velpeau, Nélaton, Guthrie, Gross, and others, agree that the portion of the artery thus strangulated is killed and must slough away."

¹ The carotid of the dog was also occluded. The ligature had slipped from its place on the horse's artery and it was permeable. There was a roughened surface at the point of deligation, due to proliferation of the endothelia. At the time of the operation the animal was plunging violently, and I do not think that I did the deligation well. Both autopsies were made in the fifth week, and the nerves had been completely absorbed. I wish to thank Dr. J. W. Baker, U. S. N., and Dr. Macgillicuddy for valuable assistance.

My own operations most *positively prove that this conclusion is not correct*, for the continuity of the artery was unbroken, although the ligature had completely occluded it and had disappeared by absorption.

Nerve tissue seems to me to be especially suitable as a ligature.¹ It is easily obtained, is very strong by virtue of its neurilemma, and is soft and cushioned, since its cylinders of neurilemma are filled with the white substance of Schwann.

The following list contains, as far as I am able to learn, all the cases in which the tape-like animal ligatures have been used upon arteries in their continuity.

CLASS A.—*Ox-aorta*.

Four double distal operations upon the right carotids and the third portions of the right subclavians. In one of these (Mr. Barwell's), at the death of the patient, sixteen and a half months after the operation, the subclavian was occluded; the carotid was only partially so. This was Mr. Barwell's first case, and he says it was tied too loosely. In another case, by this surgeon, the patient died thirty hours after the operation, from pulmonary complications.

One right subclavian (recently tied). Result not yet known.

One right carotid. Successful.

One left carotid. Successful.

Two external iliacs. Successful.

Four superficial femorals. Successful.

One popliteal, above and below a traumatic aneurism. Successful.

One brachial. Successful. In this case catgut had been used and had failed, necessitating the use of ox-aorta for the arrest of secondary hemorrhage.

¹ In order to prevent any possibility of slipping, I tie the two ends of the knot together with a catgut.

This gives a total of 19 cases.

CLASS B.—*Kangaroo tendon.*

1. Double distal, Pollock.¹ Right subclavian, third division. Right carotid.

In tying the second loop of the knot upon the carotid the tendon broke, but the first knot held the artery occluded. Fearing it might be insecure, a catgut was applied to this vessel. Patient died ten days after operation. Both arteries were found occluded after death. Ligatures undergoing process of absorption.

2. External iliac. Successful.

CLASS C.—*Nerve ligatures.*

Right carotid. Successful.

Omitting Mr. Barwell's case, which died thirty hours after the operation; the right subclavian, the result of which is not yet obtained; and the carotid in Mr. Pollock's case, in which the tendon broke and was reinforced by catgut, there are twenty large arteries which have been tied with tape-like animal ligatures. In no case has hemorrhage occurred, and all have resulted favorably, except one, in which the operator purposely did not draw the ligature tightly.

I conclude this article with the following extract from a letter recently received from Mr. Barwell.

"I have no doubt you will find nerves, or any other fresh animal substance, act well. We know that all the soft connective tissues, as tendon and cellular membrane, possess this faculty of becoming absorbed or incorporated in the tissues, provided they are perfectly fresh. The extensor tendons of a kangaroo's tail, tendon from the whale, decalcified bones from quadrupeds and birds, have all been introduced into the human body, and have thus acted in the

¹ *Medico-Chir. Trans.*, vol. lxiv, p. 231.

tissues. In the selection of a ligature the important points seem to me to be that the substance should be absolutely free from all taint of decomposition, and that it should be cleansed from any accidental impurity by a short immersion in some antiseptic fluid. If these essentials be secured, the choice of a particular substance is immaterial; sufficient strength, persistency to keep a safe knot, as also facility of acquisition, are the main considerations."

THERAPEUTIC CONTRIBUTIONS.

V.

ON THE EFFICIENT DOSAGE OF CERTAIN REMEDIES USED IN THE
TREATMENT OF NERVOUS DISEASES.*

By E. C. SEGUIN, M.D.

II.—CRYSTALLIZED ACONITIA OF DUQUESNEL.

(*Aconitia*.—U. S. P.)

Doses as given by authorities on materia medica and therapeutics :

STILLÉ and MAISCH, National Dispensatory (1879), p. 101.¹ Primary dose $\frac{1}{250}$ grain two or three times a day. It is recommended in doses of $\frac{1}{125}$ grain.

WOOD, Therapeutics (1880), p. 180, makes the truly astonishing statement that : “The alkaloid is officinal, but, on account of its intense activity, should not be given internally.”

This was printed more than a year after the publication of the New York Therapeutical Society's report on aconitia in the *New York Medical Journal* for 1878.

* This article is a continuation of one which appeared with the same title in the April number of this journal, page 177.

¹ In this paragraph occur several serious misprints. The dose of $\frac{1}{125}$ grain is rendered as gm. 0.005, which is really $\frac{1}{125}$ grain, and might prove fatal. In speaking of external applications, 2 and 5 grains are rendered as gm. 0.133 and 0.333, which are correct figures ; but immediately after it we find one (1) grain rendered as gm. 0.666, in reality nearly eleven (11) grains.

BARTHOLOW, *Materia Medica* (1880), p. 44, simply quotes the New York Therapeutical Society's formula. No personal statement as to doses.

RICE. *Posological Tables* (1879), p. 5: "Aconitia; aconitine. Alkaloid from aconite. The commercial product is an impure mixture of alkaloids. The dose is $\frac{1}{160}$ to $\frac{1}{130}$ grain, increased with caution. Chiefly externally."

NOTHNAGEL and ROSSBACH. *Arzneimittellehre* (1878), p. 721. Aconitia is little employed internally. Dose, gm. 0.004 or $\frac{1}{16}$ grain, and the daily quantity as gm. 0.03 or $\frac{1}{2}$ grain.

This cannot refer to Duquesnel's aconitia. It might be a safe guide for giving Merck's aconitia, which is very impure and of doubtful efficacy.

GUBLER. *Leçons de Thérapeutique* (1877), pp. 147-8. Prof. Gubler may be considered as the introducer of Duquesnel's aconitia. In articles, besides in this book, he was the first to indicate its wonderful efficacy in neuralgia, particularly trigeminal neuralgia.

He recommends gm. 0.0005 ($\frac{1}{130}$ grain), or less at first; gradually increased to gm. 0.002—0.004—0.005 ($\frac{1}{30}$, $\frac{1}{16}$, $\frac{1}{12}$ grain).

Doses recommended by clinicians.

As Duquesnel's aconitia has been known so few years, and has been in use less than four years in this country, it is not singular that our principal text-books do not speak of it. Still one is surprised to find that Prof. Flint in the last edition of his "Practice," dated 1881, does not refer to aconitia among the remedies which may cure neuralgia.

HAMMOND, *Diseases of the Nervous System* (1881), pp. 857-8, speaking of the treatment of neuralgia, recommends Duquesnel's aconitia in doses of $\frac{1}{120}$ grain, gradually increased to $\frac{1}{8}$ grain if necessary, till relief be obtained, or till the characteristic peripheral numbness occurs.

Personal experience. Influenced by Prof. Gubler's article and by his book, I began using the aconitia of Duquesnel in the winter of 1877-8, with most gratifying results. More of the drug was imported, and in a few months several of my friends were trying the remedy—among them I may name Dr. McBride and Dr. Andrew H. Smith.

At a meeting of the Therapeutical Society of New York held October 11, 1878, I presented the report of the Committee on Neurotics of that Society upon the use of this aconitia. We reported ten cases cured or relieved. This report will be found in the *New York Medical Journal* for December, 1878.

Since that time aconitia has been used by many physicians in numerous cases of trigeminal neuralgia, with very favorable results. A large proportion of cases have been cured, and some very ancient cases (8 to 12 years) greatly relieved by the medicine. A few cases only have been uninfluenced.

In the last two years the alkaloid has been offered in pillular form by several reliable drug-firms, and I can testify to the potency and reliability of Caswell & Hazard's tablets, and of Schieffelin's pills. These firms furnish doses of $\frac{1}{200}$ grain and of $\frac{1}{100}$ grain.

In my first use of aconitia I employed a solution made by the late Dr. William Neergaard, the only pharmacist who then (1877-8) held a sample of Duquesnel's preparation. My formula was :

℞		
Aconitiæ (Duquesnel's),	gr.	$\frac{1}{10}$
Glycerinæ,		
Alcoholis,	āā	3 i
Aquæ menth. pip.,	ad	$\frac{3}{4}$ ii

Each teaspoonful (estimating seven teaspoonfuls to the ounce) contained about $\frac{1}{140}$ grain. This dose was to be given two, three, or more times a day, on an empty stom-

ach, till the pain ceased or the physiological symptom—numbness—was produced. As my subject to-day is not clinical therapeutics as much as posology, I pass by many interesting facts about the use of aconitia and omit all cases.

The remark which I have already made about the necessity of giving small doses of potent drugs to a patient whom we see for the first time, and of estimating his susceptibility, applies with especial force to aconitia. Bearing this in mind and carrying it into practice we may be very bold, almost rash, later on, without running real danger.

Those of us who introduced aconitia in 1878 soon discovered that some persons, females especially, were powerfully affected by minute doses. Dr. A. H. Smith reported a case to our Committee in which a lady was distressed by $\frac{1}{400}$ grain, and I myself, while in a reduced state of health and suffering severe trigeminal pain, was severely benumbed by $\frac{1}{200}$ grain (though long afterward, when quite well, it required two doses of $\frac{1}{100}$ grain to produce nearly similar effects).

It is well, consequently, to give debilitated, susceptible, and female patients, doses of $\frac{1}{250}$ or $\frac{1}{200}$ grain (from gm. 0.0002 to gm. 0.0003) to begin with. These facts have induced the Messrs. Schieffelin & Co. to cease making pills of $\frac{1}{100}$ grain, and to furnish only the $\frac{1}{200}$ grain which can be repeated at will. Messrs. Caswell & Hazard still furnish both doses in the shape of soluble tablets.

In a case of neuralgia, after a day's testing with minute doses, if I find no undue susceptibility to the drug I give it freely— $\frac{1}{100}$ grain every 3 or 4 hours until distinct numbness and coldness (subjective coldness) be felt in the limbs and face. Then a longer interval may be allowed before giving another dose. Some subjects will take 3 or 4 tablets of $\frac{1}{100}$ grain, each day, and be in a constant state of numbness without harm, and often with curative effect.

In some of my cases of chronic epileptiform neuralgia I have kept patients under the influence of the drug for days and weeks,—and have seen no evidence of cumulative effects.

As a rule, in testing a man of average physical development and not reduced by disease I at once start with doses of $\frac{1}{100}$ grain (gm. 0.0006).

As regards maximal doses, I may state that in certain cases of posterior spinal sclerosis with severe fulgurating pains I have given from 4 to 8 doses of $\frac{1}{100}$ grain each in 24 hours, producing in some cases faintness, sickness, and a considerable prostration. I might add that this form of nerve pain has never been relieved by aconitia, and that with hardly an exception, all the tabetic patients I have experimented on have not shown any trace of the numbness which is *the* sign of aconitia effect in healthy persons.

As a rule, the pain of trigeminal neuralgia ceases when the physiological effects of the drug are manifest. I do not pretend, and Prof. Gubler did not claim, that aconitia is a certain or specific remedy against trigeminal neuralgia, but it certainly is the best of all our present therapeutic resources against this terrible disease. Of course in certain cases, special etiological factors must be considered, and other treatment given besides the aconitia: for example, in clearly malarial neuralgia, and in syphilitic neuralgia, or in the (rare) neuralgia from bad teeth.

III.—PHOSPHORUS AND PHOSPHIDE OF ZINC.¹

(*Phosphorus*.—U. S. P.)

Doses given by authorities on *materia medica* and therapeutics:

STILLÉ and MAISCH. National Dispensatory (1879), p.

¹ The equivalent of zinc phosphide ($Zn_3 P_2$) is $195.6 + 62 = 247.6$. Consequently one part of the phosphide contains 25 % (about) of phosphorus.

1072. These authors, apparently wholly relying upon Gubler and Thompson, state that the dose varies from $\frac{1}{30}$ to $\frac{1}{4}$ grain. They say: "Those who have most advocated its use recommend that a first dose of one eighteenth of a grain (gm. 0.003) should be repeated every four hours till six doses are taken. If then no improvement (in neuralgia) have occurred, the dose should be increased to one twelfth of a grain (gm. 0.005), and repeated in the same manner as before."

They do not, however, mention Thompson's alcoholic solution of phosphorus.

Zinc phosphide (p. 1546) in doses of $\frac{1}{16}$ to $\frac{1}{8}$ grain, and even $\frac{1}{3}$ grain.

STILLÉ. Therapeutics (1874), vol. i, p. 800. "Moderate doses of $\frac{1}{40}$ to $\frac{1}{4}$ grain." (Phosphorus.)

WOOD. Therapeutics (1880) p. 113. Recommends a mixture containing oleum phosphoratum, each dose to contain from $\frac{1}{30}$ to $\frac{1}{15}$ grain; or of a chloroformic solution in a mixture, $\frac{1}{16}$ grain.

The dose of zinc phosphide he gives as $\frac{1}{100}$ to $\frac{1}{50}$ grain, which is in strange contradiction to his full doses of phosphorus.

BARTHOLOW. Materia Medica (1880), p. 96.

Dose of oleum phosphoratum, U. S. P., 5 to 10 drops (equal to $\frac{1}{24}$ or $\frac{1}{12}$ grain, as each π of Ol. P. = gr. $\frac{1}{120}$ of P.).

Quotes Radcliffe's formula for pil. phosphori, $\frac{3}{100}$ grain in each pill. Also quotes Thompson's tinctura phosphori in doses equivalent to $\frac{1}{40}$ and $\frac{1}{20}$ grain.

The dose of phosphide of zinc is $\frac{1}{12}$ to $\frac{1}{4}$ grain.

RICE. Posological Tables (1879). Oleum phosphoratum (p. 54). No dose given. Recommends Dr. Squibb's solution: Phosphorus, 1 part; cod-liver oil, 99 parts.

"Phosphorus, $\frac{1}{100}$ to $\frac{1}{20}$ grain, increased with caution."

NOTHNAGEL and ROSSBACH. *Arzneimittellehre* (1878), p. 200. Dose from $\frac{1}{63}$ to $\frac{1}{12}$ grain (gm. 0.001—0.005).

GUBLER. *Leçons de Thérapeutique* (1877), pp. 236–7. Dose, gm. 0.001 ($\frac{1}{63}$ grain) in granules; from 2 to 10 a day.

Praises the oleum phosphoratum in capsules.

Zinc phosphide, from $\frac{1}{6}$ to 1 grain (gm. 0.01—0.05) *per diem*; he rather depreciates its virtues.

Doses as given by authorities on clinical medicine:

HAMMOND. *Diseases of the Nervous System* (1881), p. 69. Speaking of cerebral congestion, he says that the oleum phosphoratum may be given in a mixture in doses of 5 drops (or about $\frac{1}{26}$ grain of phosphorus).

Zinc phosphide, the formula of which he gives as $Zn_3 P_2$, and estimates as containing $\frac{1}{7}$ of phosphorus, he recommends in $\frac{1}{10}$ grain dose, in pill form (this gives $\frac{1}{70}$ grain of phosphorus); or the phosphoretted resin may be used to make pills, each containing $\frac{1}{50}$ grain of phosphorus.

FLINT. *Practice of Medicine* (1881), p. 797. Merely names phosphorus as a remedy for neuralgia; gives no doses or estimate of its value.

ANSTIE. *On Neuralgia* (1871), p. 180. States that he has used the phosphuretted oil and pills of phosphorus (Dr. Radcliffe's), containing $\frac{1}{30}$ grain, three times a day. He does not estimate it as specially useful. (This was written before the publication of Thompson's work.)

J. ASHBURTON THOMPSON. *Free Phosphorus in Medicine*, London (1874), p. 190: "The chief precaution to be observed in the treatment of neuralgia with free phosphorus

* * * is to administer a full dose of the remedy in the first place."

" * * * unless half a grain or more be given in the course of each twenty-four hours, frequent failures, or only partial successes in treatment will be met with." "But the remedy must be given in not less than this dose, *i. e.*, one

twelfth of a grain repeated every four hours, from the beginning of treatment."

Page 191. He admits the utility of the alcoholic and ethereal solutions, reduced phosphorus, and even zinc phosphide, but he has had the best results from one twelfth of a grain of phosphorus dissolved in cod-liver oil, every four hours.

Thompson has more recently furnished the following formula for the preparation of a solution of phosphorus, which is not unpalatable to most patients.

Take of

Phosphorus,	gr. i
Absolute alcohol,	3 v
<i>Dissolve with heat.</i>	
Glycerine,	3 xii
Alcohol,	3 ii
Essence of peppermint,	℥ ii.

Mix the two solutions, which make nearly 3 xx; 3 i = $\frac{1}{20}$ grain. This should be given without water.

Personal experience. Very soon after the appearance of Dr. Thompson's article, I caused this solution of phosphorus to be made by Mr. F. Haas, by Caswell, Hazard, & Co., and by the late Dr. Neergaard, and used it a great deal. A weaker preparation or imitation, under the name of elixir of phosphorus, one teaspoonful of which contains $\frac{1}{40}$ grain, is also sold, but I prefer the stronger form, and write for *solutio phosphori* (Thompson).

I have employed this solution with the greatest success in trigeminal neuralgia, and with some success in other neuralgias—following Thompson's plan of giving full doses, usually 1 teaspoonful (about $\frac{1}{8}$ grain, if we estimate a teaspoonful to be a little over 3 i), every 3 or 4 hours. I have known a severe facial neuralgia (not chronic epileptiform neuralgia) cured in two days, and even in 24 hours; several cases in a week.

In conditions of nervous prostration, cerebral anæmia, in-

ipient cortical degeneration (dementia), in melancholia, I have been much pleased with a combination of Thompson's solution and cod-liver oil in the proportions of 1 : 6 or 1 : 4, a tablespoonful of the mixture being given after each meal.

In other cases I have had an extemporaneous mixture made and given two or three times a day : Thompson's solution, 1 teaspoonful ; sherry, 2 tablespoonfuls ; cod-liver oil, from 1 to 2 tablespoonfuls ; and the yolk of one egg, thoroughly beaten and mixed, with the addition of a little extra oil of peppermint. This is well received by most patients, and constitutes a most valuable tonic.

The phosphide of zinc in doses of $\frac{1}{6}$ to $\frac{1}{4}$ grain (gm. 0.01 to 0.0125) combined with *nux vomica* or with *belladonna*, according to indications, has seemed of some efficacy in the treatment of posterior spinal sclerosis, of cerebral anæmia, of nervous prostration ("neurasthenia"), and of incipient dementia.

With pills of pure phosphorus I have had little experience. The pills offered by most of our manufacturing drug-concerns are of too small a dosage. As may be seen from the citations made, and from my own experience with other preparations, the giving of $\frac{1}{100}$ grain (gm. 0.0006), or even of $\frac{1}{50}$ grain (gm. 0.0012), of phosphorus is of probably very little use. From $\frac{1}{30}$ to $\frac{1}{10}$ grain (gm. 0.002—0.006) should be administered three times a day, with, of course, due watchfulness for signs of gastric irritation.

IV.—CRYSTALLIZED NITRATE OF SILVER.

(*Argenti nitras.*—U.S. P.)

Doses as given by authorities on materia medica and therapeutics :

STILLÉ and MAISCH. National Dispensatory (1879), p. 237.

Dose from $\frac{1}{6}$ to $\frac{1}{4}$ grain, three times a day. Doses of $\frac{1}{2}$

grain occasion no special symptoms, but larger quantities are apt to cause gastric heat, pain, and nausea.

STILLÉ. *Therapeutics* (1874), i, p. 367, *et. seq.*

Quoting authors upon diseases of the nervous system he refers to doses varying from $\frac{1}{8}$ to $\frac{1}{4}$ grain, three times a day.

WOOD. *Therapeutics* (1880), pp. 51-3.

Dose from $\frac{1}{4}$ to $\frac{1}{2}$ grain, in pill form, given upon an empty stomach, when it is desired to affect the stomach, and after meals, when the constitutional effects of the drug are desired.

“When given for a chronic disease, its administration should be suspended for one week, at the end of every third week, and its employment should not extend over a longer time than three months, without a protracted intermission.”

BARTHOLOW. *Therapeutics* (1880), pp. 213-5.

Gives elaborate directions and formulas for its use in various visceral affections, dyspepsias, gastritis, colitis, etc., but hardly refers to its use in nervous diseases, and does not recommend it.

RICE. *Posological Tables* (1879).

Dose $\frac{1}{4}$ to 2 grains: increased with caution.

NOTHNAGEL and ROSSBACH. *Arzneimittellehre* (1878), p. 113.

In pill-form, gm. 0.005—0.03 ($\frac{1}{12}$ to $\frac{1}{2}$ grain).

GUBLER. *Leçons de Thérapeutique* (1877), p. 579.

Thinks that no good effects are to be expected from its internal use, and gives no doses.

Doses as given by authorities on clinical medicine:

The older English physicians, Sims, Wilson, Harrison, and Roget, quoted by STILLÉ, *Therapeutics*, i, p. 367, gave doses of one, two, three, and even six grains, three times a day, for epilepsy. It is not now employed for this disease, I believe.

FLINT. *Practice of Medicine* (1881), p. 476.

Speaking of locomotor ataxia, he recommends giving from $\frac{1}{8}$ to $\frac{1}{3}$ grain, three times a day, for several weeks ; then suspending its use for a while.

HAMMOND. Diseases of the Nervous System (1881), p. 633.

In the treatment of locomotor ataxia, merely mentions dose of $\frac{1}{4}$ grain three times a day ; gives no details, and does not seem to attach any value to the drug.

WUNDERLICH, *Archiv der Heilkunde*, 1861, ii, p. 193 (cited by STILLÉ, p. 368), gave $\frac{1}{3}$ grain twice and thrice a day ; quoted by TOPINARD, he gave $\frac{1}{6}$ grain three or four times a day : for locomotor ataxia.

BOUCHUT (cited by STILLÉ, pp. 368-9), *Bull. de Thérap.*, lxiv, p. 57, gave to a child 5 years old, with paraplegia, $\frac{1}{10}$ grain twice a day. To adults for paralysis from $\frac{2}{3}$ to 1 grain a day.

TOPINARD, *De l'ataxie locomotrice*, Paris, 1864, pp. 435-468, gives a full account of the attempts to cure sclerosis of the posterior columns by silver ; relates several cases of his own, and concludes that the drug is usually useless in locomotor ataxia ; he gave from $\frac{1}{6}$ to $1\frac{1}{2}$ grains *per diem*.

He gives the following doses as prescribed by several well-known physicians.

CHARCOT and VULPIAN in 1862 gave doses of $\frac{1}{6}$, $\frac{1}{3}$, $\frac{1}{2}$ grain three times a day. Later CHARCOT has given as high as $1\frac{1}{3}$ grains in a day.

PIDOUX, $1\frac{1}{3}$ grains *per diem*.

GUBLER and BEAU, $1\frac{1}{3}$ gr. *per diem*.

HILLAIRET, $2\frac{1}{2}$ gr. *per diem*.

W. ERB. Ziemssen's Cyclopedia, Am. ed., vol. xiii. On Diseases of the Spinal Cord, pp. 614-5. Recommends from $\frac{1}{6}$ to $\frac{1}{3}$ grain three times a day, or from 1 to $1\frac{1}{2}$ grains *per diem*, until 120 or 150 grains have been consumed. He has a high opinion of the medicine, for he says, p. 614 :

“Among the *internal remedies* for tabes, nitrate of silver undoubtedly stands first, as it can show quite undoubted results.”

Personal experience. I have employed nitrate of silver extensively in the treatment of locomotor ataxia, and am almost disposed to agree with Erb. I can most positively assert that in quite a number of my cases the course of the disease has been materially checked, and in many others repeated periods of relief secured by nitrate of silver.

I have also given it in various forms of subacute and chronic myelitis, but with less definite results; though my impression of its action in these cases is favorable.

I seldom prescribe less than $\frac{1}{4}$ grain (gm. 0.0125) of silver at a dose, and usually give $\frac{1}{2}$ grain (gm. 0.03). The remedy is made up in a pill with an indifferent extract (taraxacum), or with extract of nux vomica, or with extract of belladonna, according to the indications of the case, whether for spinal stimulation or for sedation.

I always give the pill before meals, three times a day, and occasionally administer a fourth pill at bedtime. A course of silver, with me, usually lasts two months, which, at the rate of $1\frac{1}{2}$ grains (gm. 0.10) a day, would give 90 grains (gm. 6.)—a perfectly safe quantity as regards danger of discoloration of the skin. After an interval of two or three months I often give another, shorter course, and so on.

None of my patients has as yet shown discoloration (argyria), and I have seldom been annoyed by the occurrence of gastric and intestinal irritation. Albuminuria I have never seen.

EDITORIAL DEPARTMENT.

A CONTRIBUTION IN RE GUITEAU.*

By HENRY P. STEARNS, M.D.,

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“ When we in our viciousness grow hard—
O misery on 't !—the wise gods seel our eyes ;
In our own filth drop our clear judgments ; make us
Adore our errors ; laugh at 's while we strut
To our confusion.”

Antony and Cleopatra, act 3d.

FIRST. We know nothing objectively of mind except as it is manifested through the brain.

Second. The brains of no two persons are, in all respects, arranged or constituted exactly alike, and, consequently, manifestations of mind as to memory, judgment, and ability to reason, will differ, in some degree, in all persons.

Third. Pathological changes in the brain and other organs of the body produce, sooner or later, *impairment* and *change* in the character of the functions of these organs, and we determine the character and probable extent of disease by studying the nature of the change.

Fourth. It, therefore, follows that in studying mind in its relation to disease, we should do so primarily by comparing

* Read before the New England Psychological Society, at its regular meeting, held in Boston, April 11, 1882.

its character as presented at the time being with that known to have existed during some definite period of its former history, and also with reference to the element of duration.

As this, however, is not always practicable, on account of our imperfect knowledge of the character and mental history of the individual, it becomes necessary, in such a case, to form an opinion on other data; and, as there exists a sort of common standard of mental action, a comparison may be made with this, and an opinion formed as to quality of mind in relation to health and disease, in this manner. In following this second method it becomes necessary to bear in mind always that the standard of comparison is one more or less variable in its character, and hence that in doubtful cases it is of the utmost importance to ally it with the first method of investigation.

In this paper it is proposed to study the evidence in the case of Guiteau in these two ways, long recognized as affording the most reliable method of examination, viz.: 1st, in relation to *change of character*, and 2d, in relation to *the common standard of mental activity*, and close with a few statements as to the results of personal examination.¹

I.—*In Relation to Change of Character.*

It appears that from his youth Guiteau was headstrong, wilful, and impatient even of parental authority, and at the age of 14 years he had a physical contest with his father; that he was unwilling to follow advice, was inconstant of purpose, and at the age of 16 was immoral, and that at 19, in the opinion of his father, he went to the Oneida community that he might the more easily gratify his sexual desires; that during five or six years of that portion of life when character is more rapidly forming, he resided in a community whose members profess to believe that a violation of those prin-

¹ As the family history and the evidence in the case are so familiar to my readers, I shall not encumber the text by reproducing them in this paper.

ciples of morality which are recognized as binding even among savage nations, is right, and hold it as a part of their religious creed and practice ; and some of whose members, while claiming to be Christians, and even inspired, yet daily tread the precepts of Christianity into the mire of sensuality, and seek to destroy those distinctions between right and wrong by which alone society is made possible.

It is in evidence that his practice of law consisted mainly in making collections, and in evading his just obligations toward those who employed him ; that he was in the habit of visiting those in prison and obtaining money without accomplishing any thing for their relief ; that he used various artifices, under the forms of law, to obtain possession of the property of others.

It is in evidence that he defrauded nearly everybody in reference to his living for years ; that he wandered about the country regardless of social or family obligations ; that he disliked his father, and did not visit him for years, and rarely visited other members of his family ; that he was more than once in prison for criminal acts, and during months in daily contact with criminals.

It appears that while professing to be a Christian and anxious to save souls, he seemed to be greatly more anxious to evade payments of obligations ; that he avoided lecturing in States where he could not escape fines for violating law ; that he made his Christian profession a cloak with which to shield himself from the consequences of violated law, and finally gave up his lecturing because "theology did not pay."

It is in evidence that he was selfish and self-seeking, and anxious for notoriety in some form or other ; that when reproved by his only brother for leading such a mode of life as he was accustomed to, for making false representations, and obtaining money and trust by false pretences, he replied

that he would not listen to such reproof longer, that he was a fighting man and would retaliate; that on another occasion, for a similar reason, he did strike his brother in the face; and further, that for years he had gone down step by step, finding it more and more difficult to obtain the means of subsistence by such methods as he had employed for years.

It is also in evidence that there had existed and been manifest from the period of his boyhood, a boundless egotism, and a consequent credulousness in reference to every thing pertaining to himself; that he had a judgment far less reliable than is common, both as to the conduct and motives of himself and other persons; that his ambition by no measure compared with his education and lack of persistent application, so that he constantly exhibited a character both unbalanced in its faculties, and inharmoniously developed.

Constant disappointment and failure in plans of life usually have a profoundly depressing effect upon minds as ordinarily constituted, but upon such as above described, they have comparatively little. The egotistical element rises above and triumphs over such misfortunes as would crush more ordinary ones, and they pass harmlessly by, while a disregard of moral obligations tends more surely to demoralize and degrade them.

Referring now to the mental characteristics of Guiteau as presented at the time of the trial, I think we may say that they are such as are foreshadowed by his previous history; that he was vastly egotistical and self-credulous, that he was selfish and self-seeking, that he exhibited malignity of character in hatred of his brother and other members of his family, that he was oblivious of moral obligation, and more so now than he was twenty years ago.

Has there, however, come in his character any other than such change as we should expect from development of

character under such surroundings and conditions of life? Bearing in mind that as years pass on all character changes in some measure, and in accordance with circumstances, education, and the efforts of will made by its possessor; that it becomes better or worse, stronger toward right and truth, or weaker toward evil, year by year, in proportion as temptations are resisted or yielded to, I am unable to conclude that any other change has come in the case of Guiteau than such as was the legitimate outgrowth of his mental constitution and his mode of life.

I certainly find no evidence that at any particular period of his life there was any such marked change as evinced a pathological condition of his brain. To assume that such a change began, or was indicated by his striking his father when 14 years of age, or by giving up his studies and going to the Oneida Community when 19, is simply begging the question. These acts by themselves are no evidence of insanity either in him or in the many others who have acted in a similar manner. Every one has to decide for himself what course in life he will pursue, and not a few have begun a course of study and relinquished it. Guiteau did so, and thought he would prefer to follow such a course of life as he thought existed in the Oneida Community. In adopting this course there is no evidence that he acted hastily, confusedly, or unadvisedly, and there is no more reason for supposing him insane on this account than for supposing others are who follow an unwise or a fanatical course in life. There is no reason to suppose that any of the Community thought him insane when he came there, or that he acted in a manner different from others who came. There is no ground for assuming that his conduct while there was other than would be likely to come from one who had any right views as to purity and virtue left, or any aspirations toward a better or different mode of life.

The very fact that he became uneasy, restless, and dissatisfied with his life there and anxious to get away evinced healthy rather than diseased mind. The fact that after he had escaped, and had come in some measure to realize his great injury, he felt indignant and revengeful, and tried to obtain compensation for loss of time, was in keeping with good-sense.

That after being buried five or six years in the moral and intellectual darkness of such a community, he should, with his innate ambition, on coming into the light and freedom of ordinary life, largely overestimate his own ability; that he should misjudge as to the public requirements in the way of religious reading, imagining that society in general was much like the little one in which he had lived; that he should indulge in projects impracticable, so far as he was concerned, is perfectly natural, and was one of the results of narrow experience and ignorance of the world at large upon such a mind as his. Moreover, it should be borne in mind that thousands, who have had higher advantages than Guiteau, have overestimated their own ability; have had visionary projects, one after another, all their lives, and yet have not therefore been insane. Guiteau's egotism was certainly remarkable, and yet his own sister and brother seemed to have almost as much as he did; and they evidently greatly enjoyed being the observed of all observers, even under the very painful surroundings of the trial; they were both ready to, and on several occasions did, attempt to put questions to the witnesses, and were prevented from doing so only by the repeated orders of the Court. I, therefore, conclude that this desire for notoriety, instead of being an evidence of any morbid change in the brain, is a trait of character native in the Guiteau family, and that Charles Guiteau has it only a little more pronounced than the other members.

Besides, it is important to remember that love of notoriety, ambition, a weak judgment, immorality of any kind, an inability to make a connected or logical speech of ten or twenty sentences, a readiness to compliment the court, the jury, or witnesses, while one thinks there is something to be gained by so doing; or a readiness to denounce them when there is no longer any thing to be gained from them; a failure to appreciate the proprieties of the court-room, or one's obligations toward friends who are trying to do their best, or any other peculiarity, or eccentricity, is of little moment except when considered in relation to *change in quality of mental operations*.

The important question is, whether, in these exhibitions of mind, there was any confusion of thought, incoherence in language, dulness of comprehension, failure in understanding, inability to know the bearing or relations of what he did and said, and of what was done and said by lawyers and witnesses, or, in any other way, exhibition of *intellectual impairment*? On the contrary, he understood so fully and so well the bearing of all that was said and done, and perceived the weak points in his case so clearly, that he again and again protested and begged that he might have more able counsel. While glancing over the newspaper he was alert and always ready with some statement, or joke, or sharp rejoinder, with the view of weakening an unfavorable impression. I think he did not denounce a single person except after he had concluded that he could get no more out of him to his benefit, or when he thought he had compromised his case. He showed that he fully realized that the issue of his case turned on the one possibility of his being able to convince the jury that he was insane at the moment of shooting. In fact, I fail to find any evidence that at any former period of his history his intellect was more strong and clear in its operations than it was during

his preparations for shooting the President, and during the trial.

Insanity becomes an exceedingly hazy and uncertain quantity when made to depend on any kind of eccentricity, or conduct, except such as evinces *failure, impairment, and change of intellect.*

II.—*In Relation to the Common Standard of Mental Activity.*

I now proceed to the second method of examination, viz., *a comparison with the common standard of mental activity.* In this proceeding it becomes necessary to select some well-known transaction, or period of mental history, by means of which to institute a comparison, and which will afford scope for the exhibition of such quality of mind as pertains to the actor. I propose, therefore, in this case to select the shooting of the President, which, perhaps, will give as severe a test of mental action as any could, and which will be better for our purpose than any other, as we are able to trace it quite clearly and fully from the beginning to the end. Our examination of this will relate, first to *plan*, and second to *motive*.

First.—As to *plan*.

Let us suppose that for any reason Guiteau had fully determined in his own mind to remove the President: the first question that would arise would be as to how it could be accomplished. A plan becomes necessary, and a moment's consideration shows that very great difficulties in the way of accomplishing such an act and escaping its immediate consequences present themselves.

1. The President could not be visited by Guiteau in the White House, as he had already been refused an audience many times.

2. The President rarely went abroad in the city alone, or

was present at any such time or place that he could be shot, except with great danger to other persons, and at the greatest immediate peril to the criminal.

3. Guiteau was unable to provide himself with the means of either flight or safety for any length of time; he had neither money nor friends of influence, nor ability to secure a place of refuge; he could not divulge his purpose to any one, lest, as he himself said, he should at once be locked up. Moreover, if these difficulties could have been overcome, the purposes he had in view in reference to any future for himself could not have been accomplished by such methods. The largest results which could have come to him from any immediate security would be a short delay in the execution of justice. If, therefore, the President is to be removed, it must be done in the broad light of day, and only at the greatest peril to Guiteau.

Under such circumstances and almost insurmountable difficulties it becomes necessary to form a plan, and one far-reaching in its bearings, if there is to remain the shadow of a hope of life afterward. How far-reaching his plan actually was and how much it involved, appears from the history, thus far, of its execution.

Fortunately, or unfortunately for Guiteau, the basis for such a plan was ready at his hand. The President was at variance with a portion of his party; the interest of a large portion of the public in the solution of the difficulty was intense. The newspapers were full of the possibilities of the situation and the probabilities of its results. Now the plan of Guiteau embraced the following points.

1. The shooting the President under such circumstances as to create the largest possible confusion in the minds of all who should be in the immediate vicinity.

2. Escaping the hands of the mob by securing the means of conveyance at once to the prison of the District,

and by asking that the prison be guarded by a detachment of the army to prevent immediate violence, thus showing that he fully considered the effect upon the public mind of such an act.

3. The allaying of all party strife in the midst of such a profound public calamity, and a consequent union of factions, and a harmony of action under the new administration.

4. The placing in power and positions a large number of persons who, under other circumstances, would not have been so benefited, and a consequent obligation arising from such personal advantages growing out of his act in removing the President.

5. A foreseeing that even such results in the way of allaying party strife, and securing union of feeling and sentiment, and individual emolument, would be inadequate to protect him from the punishment of his crime, and, therefore, that something additional must be pleaded in extenuation. This was devised from an experience which had for years been familiar to the mind of Guiteau, viz.: an *inspiration from Deity* to do the act.

On this part of the plan all the other details hinged. Except for this, the others would be of comparatively little use, and for this all the other portions were arranged: To escape the mob, get behind the prison walls, and wait for the results which were sure to come in change of administration and office, and in the public mind, and gain an opportunity to plead in defence an *inspiration from Deity*, which so overpowered his mind as to free him from responsibility.

In presenting this last point as a part of the original plan, I have not forgotten that the prosecution distinctly took the ground in the trial that the idea of inspiration was all an after-thought, but from my conversations with Guiteau

during four interviews, I became strongly impressed with the view that the inspiration idea was a part of his original plan. This was confirmed during a fifth interview when, in reply to my question as to when he had *first* told of the inspiration, he said he was *positive* he told Detective Brooks, when he saw him on the night of July 2d, as "it was a distinct part of the idea as it lay in my mind." It was also confirmed by reference to the telegram sent to the *Boston Globe* on July 3d, which contained the same statement, and again by Dr. Gray's testimony on the stand, that Guiteau had admitted that it was a part of his plan at his first or second interview with him in prison.

The above plan exhibits as in full activity, memory, reason, self-control, perception, a full appreciation of the character of his act as it would be regarded by the public, and its immediate effect upon those who should be present, and remarkable judgment in its provision against personal consequences.

Now it is a well-recognized fact that sometimes insane persons make and execute plans, and, therefore, the importance of a plan as a criterion of insanity in a given case is great, chiefly as it may evince defect, or disease, of some faculty of the mind. This is the case generally with the plans of the insane if they are complicated, and they rarely go beyond the accomplishment of the deed, or embrace its consequences so as to provide against them except in an imperfect manner. An insane person who is maniacal, or controlled by delusions, may, for instance, make careful arrangements to escape from an asylum, or to kill his attendant or physician; he may display much cunning in his plan, but he rarely, if ever, goes further than to escape, or shield himself by denial, while all beyond is left to chance; and I think it may be affirmed that never in the history of the insane has one planned the execution of a criminal act in con-

sequence of his insanity, with the *intention beforehand of pleading his insanity as the ground of his irresponsibility.*

The difference, therefore, between the plan of Guiteau and those of the insane, is that it is complicated, and yet clear, connected, and far-reaching in its provisions, and evinces a full appreciation of consequences, while the latter are generally limited or imperfect, do not embrace consequences, and exhibit some impairment of ability to reason, beyond what had previously existed.

Second. We next proceed to the study of *motive*.

That we may be able to understand the significance of motive in any case, it becomes necessary to reason from the standpoint of the actor. The motives for criminal action in different persons are as diverse as actions themselves. It seems improbable in the extreme that a person should break into another's house and kill him for the few paltry dollars he might chance to obtain by so doing ; or that a vagabond should burn the house of one who had befriended him, because he had refused to do so longer, and yet the criminal calendar presents the history of many murders and arsons evincing even less ground for motive.

In the case before us, therefore, it becomes necessary to study the motive from the position of Guiteau, in order to understand how much it might signify, and to do this we must refer to his history and movements prior to the act.

It is in evidence that he had years before been interested in a political campaign, and had in mind to secure a mission to a foreign country, had his candidate been successful. In 1880 he had associated himself immediately with the National Committee of the Republican party, and daily visited their headquarters during the campaign. He had received more or less attention from members of that committee, especially the chairman,¹ and his name was down as a

¹ This statement was made to the writer by the Chairman of the Committee.

speaker should his services be called for. The fact that he was daily present at the rooms, and was recognized even in a casual manner by those present, led him to feel that he had a part in conducting the labors which were finally crowned with success, and when the election was over he went on to Washington with the crowd of others who sought office.

His address was good, his appearance was gentlemanly, he could use language fluently, and regarded from his standpoint of view he was as much entitled to an office as a reward for services, or as a personal favor, as others were. Indeed, his heart was set upon it ; he had lost in 1872, but now his party was triumphant, and there could be no failure in his sanguine estimation.

He made his application for the office in due form ; he visited the White House ; he visited the Secretary of State, and was treated with respectful attention. His requests were not granted, but neither were they denied. He was put off with some evasive reply or other, day after day and week after week. In the meantime his condition, financially, was becoming more and more critical ; he was without money or ability to obtain any ; it became necessary to come to some understanding on the matter, and hence his last interview with Mr. Blaine, when he says he was given to understand that if the President would give him the office no objection would be made. He then determined to see the President and have the matter decided once for all ; but an audience was refused, and he resorted to the last means of reaching him, and wrote a letter, but with no effect. His last chance was fading away, and the visions of this office he had so long and so assiduously sought, were becoming more and more dim and hazy to his view ; and all, not because the Secretary of State was unwilling, but because the President was unmindful and ungrateful.

What now is his situation? He is penniless; he has staked his last dollar on securing the mission. He has for months fed upon the crumbs which have fallen from the political table, and upon the words of recognition which have fallen from the lips of those so high above him. There now remains nothing for him but to return to his old and wretched mode of life, wandering about the country selling his book or soliciting insurance, and dead-heading railroads and boarding-houses. How great the contrast as between this and the experience of the last few months; and all, because this miserable, ungrateful President will not give him an office, which he is confident he is as well fitted for, and as much entitled to, as another.

He is a careful reader of the papers, and near this period of his waiting and discouragement, he sees that this author of his misery and disappointment is beginning to be denounced as one who is proving to be ungrateful and unmindful toward the claims of others as well as himself; he waits, and reads, and waits on, and his cause of grievance grows with that of others. This man, who has been raised to the highest position in the gift of the people by the influence of certain men, has turned upon them; he is exhibiting "the blackest ingratitude toward the very men who made him." Certain newspapers denounce him in the severest words; certain men refuse to speak to him even, and some say he is likely to ruin the party; others, that he is the only person in the way of the continued success of "the grand old Republican party," and he is pursuing a course which will destroy it.

He tells us himself that while reading these denunciations of the newspapers and their prophecies of evil, and meditating upon his own disappointments, the idea came to him one night to remove the President. It came to him again and again, and he harbored it; he thought it over;

he read the papers and thought more of it. It would be the removal of that man only who had stood between him and his desires, and had so miserably thwarted them. Others wanted to be rid of him as well as he, or so, at least, he interpreted the newspapers, in the desires and ponderings of his own heart. If it was done, that portion of the party with which he had always been most in sympathy would come into power and office, and he, *he alone*, would be the author of it all. In his greedy imagination which had so long dwelt upon them, these offices must have the same charms for others as for himself, and those who should prove to be so fortunate as to obtain them, and through his act alone, could not fail to condone the means, whatever they might be, by which they obtained them.

Then, his act would at once lift him up to the gaze of the country. The very pistol which he used would ever afterward be exhibited in the national library, and gazed upon by unnumbered thousands, and his name would go "thundering down the ages" as the man who had saved the "grand old Republican party."

Here, then, we have a personal motive of twofold nature: 1, *revenge*; and 2, *notoriety*. The same spirit which had, a short time before, led him to strike his brother in the face for reproving his lying and utterly worthless and dishonest mode of life, led him to strike down the man who had been the cause of his bitter disappointment and failure in securing office. It would be the removal of the man who had dashed to the ground the hopes and expectations of many others as well as his own; and, moreover, also the man whom many of the great leaders of the party never desired to have, and for whom Conkling "did not care that,"—snapping his fingers;—"neither did any of us; he made the fight for Arthur, as we all did."

The same spirit of egotism and love of notoriety which

had led him to wander from one end of the country to the other in imitation of Mr. Moody, that he might keep himself before the public and be the central personage and observed even of a small number of persons, led him in this case also to take the chances. His condition was a desperate one; to drift back once more to the miserable, laborious life he had so long followed, and with such humble results, after he had once tasted the sweets of crumbs from the public table, would be worse to him than even death itself, when it should come in a manner which would so lift him up to the public gaze. Still, as against this last issue of his undertaking he believed he had most carefully provided in his plan. While, therefore, Guiteau had never been guilty of such a monstrous crime before, yet his manner of life, his utter disregard for moral obligations, his yielding to temptation toward evil-doing, his casting off the most sacred social obligations in a criminal manner, and making oath to it in a court of justice, his whole manner of life being one of selfishness and a seeking for notoriety,—all tend to show that for years his course had been one tending toward a condition which prepared him to openly violate law in any of those ways through which temptation might approach him.

Of these two motives I have no doubt the latter one, viz.: love of notoriety, was much the stronger in the mind of Guiteau, though there was enough of the former to lead him on. Moreover, the love of notoriety has little if any influence with the insane. Persons whose brains are so much diseased as to lead to homicide, are not in a condition to be much influenced by it.

But it may be said that the fact that Guiteau had no more appreciation of his unfitness for an important official position was evidence of insanity. In reply I need only remind my readers that lack of intelligence and overestimation of

one's ability are common enough with the sane as well as the insane ; ignorance is common alike to both classes of persons ; improbable beliefs, incorrect reasoning, eccentricity of conduct, peculiarities of manner and language, claims for public positions by unqualified persons, in this country, are all common enough among the sane, and their importance as criteria of insanity is great only as they may evince degeneration from a condition of greater intelligence and a higher order of conduct at a former period of life ; and there is no evidence that Guiteau ever did have any better or higher intelligence.

In view of the above considerations I am unable to find that Guiteau was moved by any other than such definite objects and considerations as operate upon sane minds when they engage in criminal acts.

III.—*Personal Examination.*

Five months after the shooting of the President, Guiteau was examined several times by me while he was in jail. As the result of these examinations, there were found no indications of important physical disease, or any such malformation of the head as might indicate either insanity or imbecility. There was a peculiarity of facial expression, particularly when excited, which was marked. This I thought due in part or wholly to a slight deviation in the axis of one eye, and pupils rather larger than usual, and also to an inflammation, of a more or less chronic character, of the conjunctivæ.

With reference to the operations of his mind, it may be said that the memory was exceptionally good ; he appeared to have a clear and definite remembrance in reference to his family relations, and the details of his own personal history, and of occurrences which took place many years before, and he gave quite a full account of these personal experiences.

He also appeared to have a full and ready comprehension of all questions addressed to him, and of their probable bearing upon his case, not unfrequently guarding, apparently, his replies to them, in a manner favorable to his desire at the time to produce an impression. Any statement or question addressed to him seemed to reach the seat of intelligence at once, and the answer was as quickly sent back. The faculty of perception was well developed and active, and his language well chosen and expressive. There appeared to be neither hallucination nor delusion; neither unnatural excitement, depression, nor dementia. Not once during some eight or nine hours which I spent with him in jail, and the larger portion of which was in conversation, did I notice an imperfect or ungrammatical sentence, or an improper word, or, in any measure, a failure to express easily and fully all that appeared to be in his mind to say on these several occasions.

His statements in reference to the assassination of the President were made readily and as if they had long been thought over by him. He did not hesitate to claim that his act had been occasioned by the conduct of the President, who "had gone back on the very men who made him," and thereby exhibited the "blackest ingratitude" toward them. This statement was made with clenched hand, in a loud tone of voice, and in an excited manner, which appeared as if put on for the occasion.

His whole manner and general bearing appeared to be those of a man in the possession of his will-power and the other faculties of mind, and who intended to persuade not only me, but also the court and the jury, that he had killed the President as a patriotic act, and while *insane*, from a pressure brought to bear upon him by the Deity. This latter point of his case he argued at length, explaining that while he heard no voice of command from the Almighty,

yet he felt a *pressure* to do the deed, and was able finally to work himself up to the point; and that this rendered him irresponsible in law.

He distinctly said he did not think he was now insane, or had been so since five minutes after the shooting, but that if he could make the jury believe that he was insane at that moment, in relation to the act, then they were bound to acquit him. His words as I have them are: "If the jury believe that I believed that I was inspired at that time to do the act, then I was legally insane and they are bound to acquit me." He also referred to the case of General Sickles, which had been tried in that very court-room, as one in which his view of the law had been adopted by the jury, and also the McFarland case, thus showing how carefully he had studied this part of his own case.

When asked what he would have done in case the President had given him the office he sought, he replied, "I suppose that would have ended the matter," though afterward, during the trial in the court-room, he denied that this would have had any effect on his action.

When asked if he should have shot the President on the morning of July 2d if Mrs. Garfield had been with him, as on a former occasion when he refrained from so doing on her account, he replied that he should not, that her presence would have deterred him.

Conclusion.—It seems to me that we are warranted in inferring from the above analysis of Guiteau's statements and acts from the 16th of May until the close of the trial, that he was at all times in the possession of his will-power and his natural intelligence; that at no time did he fail to understand that he was preparing to commit an act so highly criminal that he would have been arrested at once if it should become known that he was planning to do it; and that at the time he did commit it he fully understood its

nature, and that he was in the greatest danger of mob violence in consequence of its heinous character. We know that he deliberated long about it before he finally decided, and that judging from his own statements as to plans and motives, he had clear and well-defined ones in his mind ; and though he was mistaken in reference to the effect of his act upon the sentiments of those who he expected would be benefited by it, yet just such results as he foresaw would come to pass as the result of shooting the President, have actually come to pass, so far as they relate to the Republican party and the general political situation, and also in rendering him one of the most conspicuous of men. We know that on two or more occasions he refrained from shooting the President, and said he should have done so on the fatal morning under certain conditions.

It, therefore, becomes certain that he was not controlled or convulsed by any diseased condition of his nervous system, which destroyed his free agency or his intelligence ; that there was no such general or particular condition of lunacy, that it overpowered any of his mental faculties so that he could not fully control and use them ; but, on the contrary, that he acted with such deliberation, such definiteness of purpose, such a foresight of consequences, both to himself and other persons, and such careful preparation for defence, as evidenced the activity of his mental faculties in their normal condition.

There is no evidence that he has ever experienced any such marked or particular change in the character of his intelligence, or in his conduct, or ability to reason, as indicated disease of brain ; or that he has ever been controlled by any delusions which he could not, and has not abandoned, when he has found them to be impracticable. In view of all the above considerations, I am constrained to come to the conclusion that his act in shooting the President was not the result of a pathological condition of his brain.

It does not, however, follow from the above conclusion that Guiteau is in all respects like other men, or like other great criminals. On the contrary, it seems to me that his general course in life indicates something quite different and exceptional. These peculiarities may perhaps be comprised under the following heads:

1. He has now and has always had an unbalanced brain, *i. e.*, one with its faculties unequally arranged or developed. He has always been greatly egotistical, self-reliant, and sanguine in reference to all that relates to himself. Such characters are not those which usually become insane. The disappointments and failures in plans and purposes of life do not usually harass and depress them, but rather pass over, leaving little effect.

2. While he has some faculties well developed, he has a faulty and weak judgment in reference to his own purposes, convictions, and motives; and also as to the motives and probable course of action in other persons. He also appears to be without that common-sense which enables persons to accurately appreciate the conditions of society, and adjust themselves thereto, so as to get on without friction.

3. While, during the earlier period of life, and under moral influences, he was not specially immoral, and never gave evidence of imbecility in this respect, yet his moral character has never been strong, and the communistic educational influences at the Oneida Community were such as to largely impair what he had. This, together with his subsequent experiences of life, has also served to destroy such feelings of deference and respect toward persons in higher and official positions of life as is common, and hence such unusual exhibitions of conduct as have made his appearance so exceptional.

4. He has had an ambition to be and do more than he

has been willing or able to qualify himself for doing ; he has been inconstant of purpose, partly from lack of mental discipline, and partly from innate quality of mind. His desire to be in some conspicuous position appears to have been so boundless as to lead him to place a false estimate on conditions and qualifications requisite, if, indeed, he ever was capable of estimating them.

5. As a consequence of this unfortunate arrangement and development of mental faculties, and still more unfortunate educational influences, and not from disease of brain, he has never been in harmony with the surroundings of his life ; he has never been willing to stay in any sphere where he might have succeeded, but has ever been reaching forth, and striving for some position for which he was not and could not be qualified. Failing in all his injudicious plans and visionary schemes of life, he has given more and more a loose reign to his bad impulses, until he has found a home within those darkened walls whence his ambition has a narrow scope.

NEW BOOKS AND INSTRUMENTS.

Diseases of Women. A Manual for Students and Practitioners. By ARTHUR W. EDIS, M.D., London. Henry C. Lea's Son & Co., Philadelphia, 1882.

This manual presents an excellent summary of the gynecological rules and doctrines considered as indisputable by the group of English and American authors from which it has the air of having been compiled. This statement embraces nearly all that can be said about the book; for it is difficult to discover in it traces of original opinion, investigation, or experience: knotty points and discussion on debateable subjects are so carefully avoided, that there is little individually characteristic offered to the reviewer for comment. We repeat, it is an excellent manual, clear, decided, sufficiently comprehensive for a beginner, extremely handy for any practitioner, safe, cautious, and precise. The book reminds us constantly of Thomas' treatise, on which it seems to have been somewhat modelled. It contains, however, a special chapter on climacteric disorders, including pseudocyesis, another on hysteria, a third on functional disorders of the bladder, and a fourth on extra-uterine pregnancy,—all subjects left untouched by the American writer. The chapter on hysteria is one of the least satisfactory in the book, the subject being really much too large for the somewhat narrow framework of the author's plan. Dr. Edis admits that for this chapter he is "largely indebted" to Reynolds' article in his "System of Medicine." He quotes, without critical discussion, first, Dr. Barnes, who "insists strongly upon the ovarian theory, proposing indeed to alter the term hysteria to that of "oöphoria"; second, Dr. Reynolds, who "maintains that it has not yet been shown that hysteria has any definite relation to the varying conditions of menstruation"; third, Dr. Graily Hewitt, who "contends that

when there is an organic cause for hysteria, that cause will be found to be a chronic flexion of the uterus."

Dr. Edis devotes several pages to a description of Dr. Weir Mitchell's rest treatment, but this is taken, not from Mitchell's own writings, but from a paper of Dr. Playfair's on the subject. The author himself has no personal experience or critical judgment to offer in regard to it.

Among the few points in the volume which may suggest special comment, we would notice: The illustration on p. 44 of the "unavoidable" dangers of tents, by a case where a laminaria tent was inserted a few days before menstruation, with a fatal result from peritonitis. This is just the time that tents never should be used, and the author would do better to condemn such a, not unfrequent, recommendation, than to use it as an illustration of "inseparable dangers," which "should deter any but those having special experience in gynecology from resorting to tents." It is probable that at least a considerable number of the accidents caused by tents have occurred in the practice of specialists.

Intra-uterine stems are advocated for the treatment of ante-flexions, with perhaps an insufficient warning of their dangers. The special danger attached to their employment in retroflexion, upon which Emmet has justly insisted, is not mentioned. Nothing could be more useful at the present day, than a discussion, supported by statistics, on the relative efficacy of vaginal pessaries and of intra-uterine stems in the treatment of ante-flexions; but this discussion perhaps would be out of place in a manual.

We scarcely think that Dr. Barnes is the "only modern author" who has discriminated between fluxion, hyperæmia, congestion, and inflammation, for Courty has been most minute and laborious in establishing distinctions between these different morbid conditions.

Chronic endometritis is said to be, in by far the larger number of cases, "practically incurable." Dr. Routh's distinction of "fundal endometritis," which we believe to be perfectly well founded, is not recognized. The caution to use nitric acid only just after a menstrual period is most necessary, and somewhat remarkable when we remember the unqualified enthusiasm for this medication once excited in Great Britain by Atthill.

We cannot agree with the author that the danger of intra-uterine injections depends on the passage of fluids through the Fallopian tubes. Much more probably do these, especially iron

solutions, penetrate the uterine glands, and thus excite colic. The chapter on fibroids contains descriptions of nearly all methods of operation, but omits Thomas' scoop, and Emmet's peculiar mode of traction. Battey's operation is spoken of very favorably, as offering "great hope," not only to sufferers from uterine fibroids, but "to a large number of women who suffer from ovarian disorders, attended with nervous manifestations of the most distressing character."

Dr. Edis' book will not help those who are searching for such profound and original suggestions as may, one day, furnish the guiding clue through the present labyrinth of gynecology. But as a summary of existing knowledge, empirical and other, it is really to be commended. [M. P. J.]

Lectures on Diseases of Children. A Handbook for Physicians and Students. By Dr. EDWARD HENOCH, Professor in the University of Berlin. New York: Wm. Wood & Co., 1882.

This excellent translation of the last edition of Henoch's lectures is really an admirable contribution to Wood's Library of Standard Medical Authors. It offers a marked contrast to the treatise by the English Dr. Ellis, issued a few years ago in an American edition by the same publishers. It contains in a compact form a very large amount of the most recent and authoritative information on pædiatrics, and this is stated so concisely, that the reader may easily fail to perceive how much has been told to him, for the very reason that it has been told so well. The author bases his lectures exclusively on his own experience; but as this is very large, he is able to cite cases illustrating not only classical and regular forms of disease, but the rare, often extremely rare, clinical varieties. Indeed, the description of individual cases is almost always devoted to the illustration of such rarities. These clinical histories are remarkable for their conciseness, and appropriateness to the point at issue.

The lectures being exclusively clinical, contain only brief directions on the hygiene and dietetics of infancy, such as usually open systematic treatises on pædiatrics. The first chapter, after a general introduction, is devoted to diseases of the new-born. The nature of icterus neonatorum is somewhat fully discussed; but nothing is said of the generalized fatty degeneration, of which a malignant form of icterus may be one of the symptoms. Trismus neonatorum is regarded, "like epilepsy, as a spasmodic disease,

specific only in appearance, and which can be produced by various irritants." Thus, baptism in cold water, hot baths administered by a "busy midwife, unable to distinguish a temperature of 33° R. from 28°" have both been causes of trismus. A case is cited where the trismus developed on the 13th day, with nephritis, the umbilicus being normal. Henoch admits two forms of erysipelas in the new-born, of which only one is related to puerperal infection. The traumatic form may start not only from the navel, but from red excoriations about the nates and genitals. The occurrence of abscesses, though indicating great danger, is not necessarily fatal. We ourselves have observed a case in which after complete convalescence and disappearance of the eruption, a six months' child suffered from a succession of subcutaneous abscesses unattended by pain, heat, or redness of the integument, and hardly disturbing the euphoria. The chapter on infantile syphilis lays due emphasis on its osseous affections. The author does not recognize any difference in the form of the syphilitic dactylitis from that of the scrofulous pædarthrocace; even the epiphyseal swellings of rachitis are only to be distinguished by their constant symmetry.

The remarkable phenomenon of complete immobility of the limbs affected by syphilitic osteomyelitis, is carefully described. Bednar, the "first author who attached any importance to this symptom," ascribed it to a myopathic affection. But Henoch explains the immobility by the pain. Cases are adduced of paresis without any discernible osseous affection, and these are explained by Wegner's discovery of a peculiar lesion in the formative zone lying between the epiphysis and diaphysis. There is excessive proliferation of cartilage cells, retarded ossification of the already calcified tissue, retarded development of vessels, with consequent deficient nutrition of cells, their shrinkage, fatty degeneration and ultimate necrosis of tissue. Waldeyer and Kobner think this process is gummatous, and that the necrosis results from compression of vessels through an excessive cell proliferation. The important clinical fact remains, that there is a diseased process at the boundary of the epiphysis, which may markedly affect the mobility of the limbs, without betraying itself by either swelling or pain.

One of the most interesting chapters in the book is that on diseases of the nervous system. Like the others it is predominantly clinical, and contains no elaborate discussions on infantile peculiarities in the anatomy or physiology of nervous organs. Many interesting clinical facts are related, especially in the section on

the usually neglected subject of hysterical affections. Among these, Henoch makes a first category of cases which many authors would certainly regard as epileptiform. "Cases in which the psychical symptoms, viz.: complete or incomplete loss of consciousness, hallucinations, delirium, predominate. * * * Consciousness is suddenly lost, the children remain standing or sitting with a fixed stare; occasionally they fall if not supported. * *

* After a few seconds, at the most several minutes, every thing is over, and the patient entirely restored. Many are unaware that they have had an attack." The author admits that the physician "can never be certain that these attacks will not degenerate into epilepsy": but it seems to us that those in which consciousness is lost may be already diagnosticated as epileptoid. Cases of hallucinations and delirium, lasting more or less continuously for two or three days, are also called hysterical. We have seen one case of prolonged liability to hallucinations and excitement, accompanied by persistent mutism, where the diagnosis, made by several neurologists, hesitated between incipient tubercle of the brain, true mania, and worms!

In a second category of hysterical affections, Henoch includes those cases in which convulsive symptoms predominate, either general or local, and those especially affecting the muscles of phonation. A third category is formed of the remarkable cases known as chorea magna. One extraordinary case of this disease is related, occurring in a girl eleven years old. It lasted five years, and exhibited the most "varied manifestations of changed nervous actions, viz.: psychical disturbances, hallucinations, and delirium, jumping and running paroxysms, opisthotonus, choreic movements, partial hyperæsthesia, and a sort of clairvoyance by which the patient was enabled to foretell the number of 'sounds' and their change of type."

Paroxysms of spasms, motions, attacks of intense local hyperæsthesia, in one case hematemesis, are reported in various clinical histories. The hematemesis was observed in a girl of eleven, prematurely developed, but not yet menstruated. It occurred on alternate nights at the same hour for ten days, each attack lasting half an hour. Henoch, having excluded simulation, having observed the occurrence of one of these attacks in the daytime, under the influence of emotional excitement, and their disappearance by purely moral treatment, ascribes them to "an irritation of the dilator nerves" of the gastric blood-vessels.

Such periodical congestions are often attributed to malarial

poisoning. Perhaps, when this etiological factor is absent, the proximate process in hysterical hyperæmias is nevertheless identical with that induced by malaria. At all events, the clinical resemblance between hysterical affections and latent or "dumb" ague is often extraordinary.

Spasmus nutans as a benign disease, due to a reflex irritant, is differentiated from the "dangerous variety of undoubtedly central origin," described by Newnham and Willshire. We had hoped to perhaps find in this chapter, reference to a form of lateral rotatory movements, occurring exclusively during sleep, instead of being arrested by it; rare symptom-complex, of which we have observed one case. But Henoeh, as well as the other systematic writers we have consulted, fails to mention this.

In the etiology of convulsions, rickets is held, we believe justly, to play a far more important part than dentition. Idiopathic contractures of the extremities are regarded as abortive convulsions, and to be entirely distinguished from tetany. They can never be produced by pressure upon the artery supplying the affected limb. The author denies the connection between spasm of the glottis and cranio tabes; and while recognizing a manifest relation between this neurosis and rachitis, considers the relation as at present inexplicable. He gives an interesting description of certain forms of pertussis paroxysm, consisting in attacks of apnoea that somewhat simulate spasmus glottidis, and which may easily prove fatal. We have recently observed precisely such a case, in a twin baby five months old, and who did succumb in an attack.

Only a paragraph is devoted to "spastic spinal paralysis," the "tetanoid paraplegia" of Erb and Seguin. It is decided to be little more than a symptom-complex, not corresponding to any definite anatomical change. "The frequently observed complication with slight mental development, even with idiocy, permits the conclusion that similar symptoms may start from the brain"; and a case is given where the autopsy showed marked structural changes of the cerebral cortex. In such cases secondary degeneration is to be expected in the cord.

A constant spinal lesion is denied as the cause of pseudo-hypertrophy of the muscles.

In the chapters on cerebral tuberculosis we find the author insisting on the frequent latency of the lesions, and on the feeble basis for regional diagnosis of tubercles. "I have often seen hemiplegia and contractures in cases in which the

autopsy showed that the corresponding parts of the brain were entirely intact, while various other parts of the cerebrum or cerebellum were the site of tubercles." Cerebral tuberculosis is held to be capable of spontaneous recovery, unless meningo-encephalitis has set in, when the case becomes hopeless. Chronic hydrocephalus is by no means always due to tubercle ; nor even always to a chronic inflammation of the ependyma of the ventricles ; but we are sometimes driven to the "unsatisfactory assumption of a fault in development, an excessive secretion of cerebro-spinal fluid." The rolling downward of the eyes in this disease is due to paralysis of the nerve supplying the superior rectus muscle, with consequent predominance of the inferior rectus, and not to presence of fluid which should produce, if anything, exophthalmus.

Henoch believes decidedly in the existence of a primary non-diphtheritic croup, "the highest development of acute laryngitis." This idiopathic croup is susceptible of recovery without tracheotomy ; but a case is cited of death in a boy of eight, nine days after subsidence of the laryngitis, from venous stasis in the cerebral veins, œdema of the pia mater, and effusion into the ventricles. In prolonged broncho-pneumonia, and also in pertussis, the author has several times seen fatty degeneration of the heart, produced by the great resistance offered to the right ventricle. He does not refer to the ingenious remark of Baginsky, that in the pulmonary inflammations of childhood, the increased resistance offered to the right ventricle is less dangerous than in adult life, because it varies less widely from the conditions normal to the age. In children the calibre of the pulmonary artery much more nearly approaches that of the aorta, than is the case in adults ; the tension in the pulmonary circulation is, relatively to that of the systemic, much higher. The right heart is therefore habituated to bear more relative strain, a circumstance which helps to explain the favorable prognosis of pneumonias in childhood. There is much more danger from their chronicity and the tendency to engorgement and cheesy degeneration of the bronchial glands, than from the cardiac paralysis which is so threatening in adult life.

In the very brief chapter on gastric dyspepsia the author refers to a form of reflex disturbance which he had previously described as "asthma dyspepticum." In an illustrative case a girl of nine began suddenly to have cyanosis : respiration of 70, moaning expiration, superficial movement of alæ nasi ; pulse small, 108. All the symptoms disappeared after the spontaneous vomiting of a hard-boiled egg.

In the chapter on tubercular peritonitis, an interesting case shows : 1st, that a severe chronic form of this disease may exist without any pain, and merely manifest itself by the symptoms of ascites, increasing emaciation, and cachexia ; 2d, that then enlargement of the liver from fatty infiltration may confuse the diagnosis. A case under our own observation confirms both these propositions. It is rather unusual to find in lectures on pædiatrics, a separate chapter reserved for diseases of the spleen, Hensch describes a simple hypertrophy of this organ, only rarely preceded by intermittent fever, and unaccompanied either by the blood changes of true, or the multiple adenitis of the pseudo-leukæmia. Recovery is not uncommon.

One of the most interesting observations in connection with nephritis, is that of anasarca without albuminuria, or sign of morbid elements in the urine during several days ; then, coinciding with the development of uræmic symptoms, the urine drawn by the catheter was found abundantly albuminous, and the autopsy disclosed an exquisite parenchymatous nephritis. The author regards these cases as at present inexplicable. It seems to us that they can be explained by a congestion of the glomeruli sufficient to seriously diminish the excretion of water and thus account for its surplus retention and effusion into cellular tissues ; while, nevertheless, the epithelium of the capsule had not yet desquamated, and therefore no albumen could escape into the tubes.

We might continue to make many other extracts from these compact lectures, but space forbids. The therapeutical sections are strongly tinged with the nihilism so prevalent in Germany, balanced, as is so often the case, by what we should consider an undue partiality for calomel. This is given even in the dyspepsia of infants, for its antifermentative action. We can see no reason for preferring calomel to gray powder in any disease of childhood, except syphilis, or, possibly, acute, simple laryngitis. Iron, cod-liver oil, and, rather strangely, morphine, are about the only remedies advised for the entire list of nervous disorders. Even chorea is held to resist all treatment, even that by arsenic,—an opinion certainly unfounded. The treatment of pneumonia, either catarrhal or croupous, is either expectant, or much more powerfully antiphlogistic than is at present usually recommended. We think in both lines of treatment the author is influenced by his rather excessively favorable prognosis in this disease. In vigorous children he applies not only dry but wet cups and leeches ; and even advises, “when careful watching is possible,” the justly

abandoned tartar emetic. There is no reason for selecting this instead of the far more manageable kermes mineral. Hensch has given up quinine in croupous pneumonia, and confines himself to local applications of cold, *infus. digitalis*, and *kali nitricum*. He lays great stress on the value of the constant cold compress, and does not seem to have compared it with the warm poultice and warm bath. For the paroxysms of pertussis, he advises only morphine, pronounces atropine too dangerous, and seems to have made no trial of belladonna,—a most excellent remedy. From the treatment of scarlatina is omitted the use of inunctions. The danger of cold baths is justly insisted upon, as also the fact, calculated to lessen intemperate antipyretics, that cardiac paralysis is threatened as much from the scarlatinous virus as from high temperature. But the amount of stimulation advised in this disease, and more especially in diphtheria, seems to us altogether inadequate.

The simplicity of children's diseases, and of their symptoms, justifies the effort to simplify their treatment; and it is certainly desirable that a severe rationalism should purge this of the polypharmacy of a former generation. But we believe the indications are more varied than those laid down by Hensch, and that more successes can be scored than he seems willing to admit.

One other remark. It seems to us a great misfortune that the orthopedics of childhood should be habitually separated from the medical treatises of their diseases, and consigned to special essays, or to works on surgery. Although mechanical principles are involved in the treatment of such diseases, the important question of their early recognition and diagnosis presents itself as a practical problem in the daily experience of every family physician. The habit of referring these diseases to the specialist or to the surgeon, is responsible for much of their neglect by these same practitioners, who, in consulting systematic lectures on children's diseases, accustom themselves to expect from them at least an outline of all the physician need be called upon to know.

[M. P. J.]

A Treatise on the Science and Practice of Medicine, or the Pathology and Therapeutics of Internal Diseases. BY ALONZO B. PALMER. M.D., LL.D., Professor of Pathology and Practice of Medicine, and of Clinical Medicine in the University of Michigan; formerly Professor in the Berkshire Medical College, Mass., and in the Bowdoin Medical College, Maine, etc., vol. i. New York: G. P. Putnam's Sons, 1882.

It has long been well recognized that the *types of the people* have a determining influence in causing variation in the manifestations of the same disease. The identical disease on the Continent frequently contrasts, rather than compares in its symptoms and in its course, when appearing in America. Within our own country even, embracing such varying climates, with its Northern cold and its Southern tropics, its new lands of the West and its old and over-crowded cities of the East, we meet with diseases in one section, which are either comparatively unknown or present marked difference in another. The appearance of American works on Practice of Medicine has been justified by the intention of their authors to present these views, and to call attention to the necessary accompanying variation in treatment, which should go with change in the type of a disease.

Based upon an experience in country practice exceptionally large, and combined with that in cities, our author has found himself in a position in which he states he can present views and direct treatment for those of his brethren who shall follow more or less in his footsteps in a country or far Western practice, which shall serve them more immediately and directly, than the foreign, or perhaps other American, works. The book before us comprises the first volume of the work. It contains, first, general considerations on pathology; then a discussion of "particular general diseases," under which are included symptomatic fevers, exanthemata, malarial affections, leucocythemia, diabetes, etc. Next follow the local diseases, and of these, those affecting the alimentary canal, the pancreas, spleen, and liver are discussed.

The general pathological observations are brief, and are based upon views held in Flint's work, and to which due credit is given. But neither these, nor the mode of presentation of the pathology, as given in any portion of this work, constitute in any way its strong feature. On the contrary, when, as on page 221, in speaking of changes in the blood, we read such general remarks as, "the blood in some cases has been found disorganized," we quote a vagueness of expression not infrequently found in this connection. It is in the thorough familiarity with disease in its protean types that our author finds his claim, not only to our respectful attention, but even admiration. Coming from this particular source we would naturally expect to see ague, one of the common affections of the far West, treated at some length and in detail; and we are pleased to note some sixteen readable pages on the treatment alone of this affection. We cannot help quoting some

remarks which, we think, should be carefully thought over by our readers. One that "cinchonism is no adequate measure of the curative effect of quinine," since minute doses produce cinchonism in some people. The practice of giving small and long-continued doses of quinine in ague is bad. Prompt and large effectual doses in ague, just as when quinine is used as an antipyretic, is strongly urged.

Among the febrile disorders we find miliary fever discussed, which, as our author states, is little if at all referred to in other works on practice. This article is based on Zuelzer's article in Ziemssen's *Cyclopædia*, from Grisolle and Jaccoud's recent works on practical medicine, and from personal statements of physicians in Rome made to the author.

Miliary fever is characterized as a specific disease, produced by a peculiar poison, but that this poison does not originate in the body, and is not multiplied in it in a manner to be communicated to others, or, at least, this is not its principal method of production and spread; that it is a disease of variable severity and course, and of uncertain termination. Its general characters, in addition to a prolonged febrile movement, are repeated sweating and the appearance of a marked sudaminous eruption. But, before concluding his article, we find that the existence of miliary fever as a distinct disease is doubted, and that sudamina and sweating are regarded as epiphenomena of certain cases of typhoid fever.

In the discussion of diabetes mellitus, which is fully given, reference is made to all the most important views in connection with the subject. Fat embolism is referred to; the subject of acetonaemia, or death by diabetic coma, being, however, omitted. A variety of rheumatism, known as neuralgic, not generally alluded to, is included. In the general treatment, the combinations of the cathartic, alkaline, and salicylic-acid treatment find most favor, and when so conjoined, are thought to be almost specific. An enjoyable chapter is found on dysentery, with its treatment considered at some length, and the use of quinine recommended as an antiphlogistic in these cases. In the treatment of peritonitis the author makes a statement to which our own experience, and the advice of Dr. Alonzo Clark, to whom the credit of the opium treatment in this disease belongs, run contrary. It is urged, when using opium or morphine, to diminish the frequency of the respirations, but not greatly. As it is thought advisable to reduce respirations, down to 12 or 15, and even 10 per minute, from 40 or 60 per minute, such a reduction must, in our opinion, be

viewed as decided. In concluding our review of the first volume, we can say that it will appeal to the class of readers for whom it was intended, and that it abounds throughout with practical observations. The student and practitioner who seek a work in which they shall find, instead of descriptions of rare pathological conditions, clear expositions of disease, symptoms, and conditions of daily occurrence, with excellent recommendations as to their treatment, will be fully satisfied by this. [H. N. H.]

ORIGINAL OBSERVATIONS.

CASE OF CEREBRAL TUMOR ILLUSTRATING THE DOCTRINE OF LOCALIZATION.

By WM. S. CHEESMAN, M.D.,

AUBURN, N. Y.

Patient, a woman aged 42. No history of injury or syphilis. She had long been subject to headaches. In January last she began to lose power over her right hand and arm, and on February 18th she completely lost the use of the hand and arm, yet did part of her work as domestic. On February 21st she attended a prayer-meeting, and there was seized with a convulsion. On recovering consciousness she was hemiplegic on the right side. She was admitted to the Auburn City Hospital February 22d, with right hemiplegia and dysphasia, though her mind seemed clear. Her condition so far improved that the paralysis of the lower limb became paresis, while that of the face disappeared. More or less contracture of the hand and arm supervened. She gradually became duller—had involuntary evacuations, vomited considerably, had several convulsions, and finally died in coma on April 11, 1882. No ophthalmoscopic examination was made.

Autopsy two hours and a half after death.

Nothing abnormal in organs of chest or abdomen.

Skull, meninges, and surface of the brain normal.

After removing the brain, a bulging was noted at the top of the ascending frontal convolution of the left hemisphere, the ascending parietal convolution being pushed backward to make room for it. To the touch this was very soft. A longitudinal section through it exposed a rounded mass of the size of an English walnut, reddish brown in color, soft and friable—the cortex

covering it being swollen and gelatinous. The section also revealed in the white substance beneath and behind this mass, a second of the same size, its lower border nearly reaching the roof of the left lateral ventricle. These were separated by a narrow partition of compromised white substance.

The right hemisphere was normal.

A microscopic examination of the specimens was made by Prof. W. H. Welch of Bellevue Hospital Medical College. In his opinion the growths were nerve-cell gliomata.



FIG. 1.—Location of tumor as indicated upon an Ecker's diagram of the convexity of the brain.

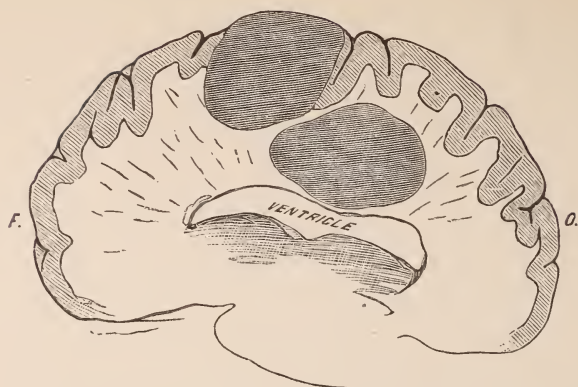


FIG. 2.—Longitudinal vertical section of the left hemisphere, showing the location of the two tumors.

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EDITED BY

E. C. SEGUIN, M.D.

S'il est possible de perfectionner l'espèce
humaine, c'est dans la médecine qu'il faut
en chercher les moyens.

—DESCARTES

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ARCHIVES OF MEDICINE.

Original Articles.

THE MYOGRAPHY OF NERVE DEGENERATION IN ANIMALS AND MAN.*

By R. W. AMIDON, A.M., M.D.,
NEW YORK.

IN as few words as possible, the object of this paper is to study, by means of careful electrical examinations and myographic tracings, the effects of peripheral nerve injury in man; alongside of which to place similar observations on frogs whose nerves have been cut, and, by knowing the histological changes occurring in the frog, to infer that not dissimilar ones exist in man when the outward manifestations are the same.

In these days, so prolific in medical literature, only the shorter, more practical articles receive careful perusal, and it is in the endeavor to make this article within the reach of all, that speculation and theorizing, details and useless histories of typical cases, will be omitted.

As a sphygmogram is a pulse-tracing, so a myogram is a muscle-tracing. By means of a mechanism, to be described later, a muscle, by its contraction, moves a lever which makes a mark on the smoked paper of a revolving cylinder. It writes indelibly on paper the following facts: quickness of muscular contraction and relaxation (abruptness of the up- and down-stroke), amplitude of the muscular movement

* Read at the meeting of the American Neurological Association, June 21, 1882.

(height and breadth of curve), and, by means of some accessory apparatus (the chronograph and the tuning-fork), the quickness of contraction after the application of stimuli (the latent period), and the effect of a continuance or a withdrawal of the same.

Any one at all conversant with electro-therapeutics will recall the retarded, slow, vermicular contractions which galvanism excites in muscles which have been cut off for some time from the spinal cord. The peculiarity of this contraction the writer thought could be beautifully demonstrated by the myograph, and it was with the idea of imitating this condition in the animal that the following experiments were performed.

In the first place normal nerve and muscle reactions were taken in healthy frogs as follows. By means of a double interruptor the circuit carrying electricity to the frog is made and broken at the same time as the circuit of the chronograph, hence the moment the electricity reaches the muscle it also reaches the chronograph and breaks the line.

Having broken up the brain and spinal cord of the frog, to stop voluntary and reflex movements, the sciatic is exposed, the tendo Achillis is cut and fastened to the myograph needle, and the frog is securely pinned to the cork stage (see fig. 1), with one electrode under the nerve and the other in some indifferent point, as the small of the back. We take the tracings produced by the cathode (neg.) closure contraction, cacc, and the anode (pos.) closure or opening contraction, ancc or anoc, excited by a small Grenet cell, and afterward the reaction to the secondary current of a Dubois induction apparatus. The same process is repeated with the muscle.

The sciatics of several frogs were cut and the animals were kept in a *very* warm room, so that nerve degeneration would not be retarded for perhaps weeks, as it sometimes is in frogs, especially in the winter.

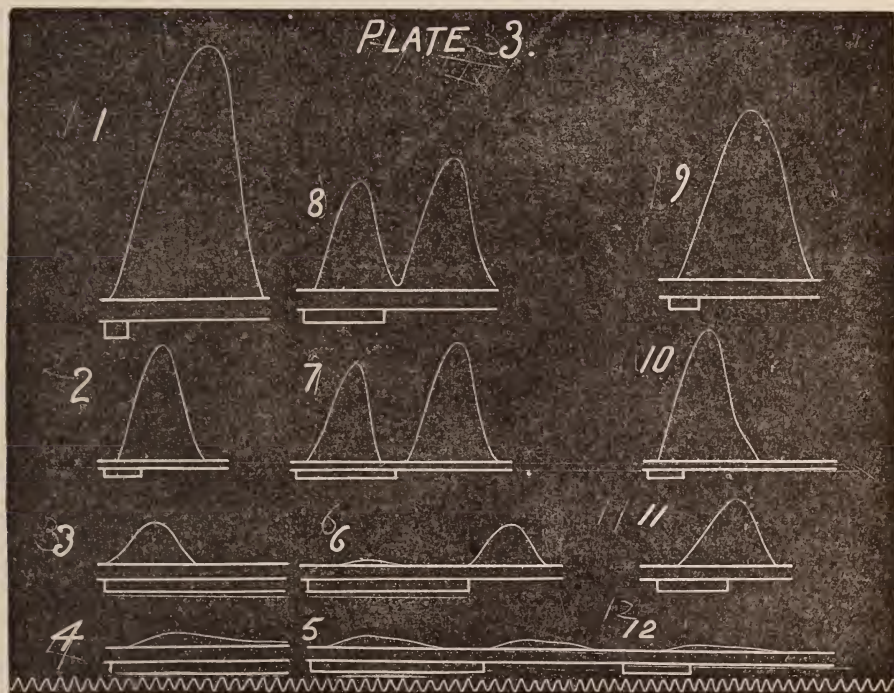


PLATE iii.—Myograms from excitation of the nerve in the frog.

FIG. 1.—Normal cacc; fig. 8, normal ancc and oc; fig. 9, normal fc, with secondary coil at 7 centimetres.

FIG. 2.—Cacc; fig. 7 ancc and anoc; fig. 10 fc, on second day of section, " " " "

FIG. 3.— " fig. 6 " " " fig. 11 fc, " third " " " "

FIG. 4.— " fig. 5 " " " fig. 12 fc, " fourth " " " "

The line on which the curves stand is called the base line, and is made by the myograph needle when at rest. The straight line below and parallel to this is made by the chronograph needle at rest, and the deviations from it denote the duration of the passage of the electrical current. The wavy line underneath is made by a tuning-fork vibrating one hundred times to the second.

Each day a frog was killed, pithed, and put through the same routine treatment as the healthy ones had been. After the electrical examination, a microscopic study of the injured nerves and muscles was also made. The formula for normal nerves is,¹ $cacc > anoc > ancc > caoc$ (the last being seldom or never seen in healthy nerves). This formula is graphically represented in pl. iii, figs. 1, 8, and 9, where the amplitude of $cacc$ is $> anoc$, which is $> ancc$, and where good muscular contraction is caused by the induced current of Dubois' secondary coil at a distance of seven centimetres from the primary coil (a current just perceptible on the tongue).

The changes in the nerve reactions following section were as follows: Forty-eight hours after section decided changes had taken place; $cacc$ had diminished two thirds, $anoc$ and cc were about the same, and faradic reaction was diminished about one third. On the the third day $cacc = \frac{1}{8}$, $anoc = \frac{1}{4}$, $ancc$ just perceptible, and $fc \frac{1}{3}$ its normal amplitude. On the fourth day $cacc$ was diminished to $\frac{1}{18}$ th, slow and tetanized; $anoc$ was diminished to $\frac{1}{12}$ th, $ancc$ the same size, and both very slow and tetanized; fc about $\frac{1}{16}$ th, retarded, slow, and tetanized.

On the fifth day of section no nerve reaction to galvanism or faradism could be detected. The Wallerian degeneration began to be very distinctly discoverable, on microscopic examination of the nerves, on the third day, increased each day, and on the fifth, and afterward, was complete, the nerves being one mass of broken myeline matter, nuclei, granular detritus, and naked axis-cylinders. This series of tracings demonstrates very beautifully the gradual death of a degenerating nerve, before only shown by the progressive diminution of electrical reactions as visible to the un-

¹ Hereafter the customary abbreviations will be used: *ca.* cathode (neg. pole), *an.* anode (pos. pole), *c* = closure, *o* = opening, *c* = contraction, $>$ = greater than, $<$ = less than, $=$ equal, *f* = faradism, *g* = galvanism, etc.

assisted eye. The nerve died early in these frogs because of the warm temperature in which they were kept. In man the nerve ceases to respond, if cut or badly bruised, on the 7th to the 14th day.

The muscles supplied by a degenerating nerve pursue a different course. The normal formula produced by direct excitation of the muscle is $cacc > ancc > anoc > cacc$ (the two last being rarely seen). This formula is graphically represented in pl. iv, figs. 1, 26, 27, where $cacc$ is one third greater than $ancc$, and where fc is produced by the secondary coil of Dubois at 4 cm. distance. Now, a word as to the degeneration of nerves in man. In the first three to four days all the electrical excitants produce simply less than normal reaction. Later (3d-7th day) fc gradually diminishes, and disappears in the second week. $Ancc = cacc$, and in second or third week becomes $> cacc$. Not only this, $caoc$ rarely, or, we may say, never seen in healthy muscle, now appears. Thus the formula is often reversed, $ancc > cacc > caoc > anoc$ (the last seldom seen). The appearance of these *qualitative* changes indicates the arrival of the Wallerian degeneration at the termini of the motor nerves in the muscle. From this time on the connective tissue increases in the muscle, cirrhosis ensues, $ancc$ becomes very much $> cacc$, $caoc$ and $cacc$ gradually disappear, and, last of all, when all muscular tissue has disappeared, $ancc$ also is wanting. Again, muscles contract more and more slowly when degenerating. Their contraction is retarded (increased latent period) and prolonged, and, in certain cases, a tetanized contraction can be produced by a continued galvanic current, which, in healthy muscle, can only be produced by a *very* strong current in a fatigued or excited muscle.

The frog's muscles after excision of the sciatics were examined as follows: Each day a new frog was killed, pithed, and tested.

PLATE 4

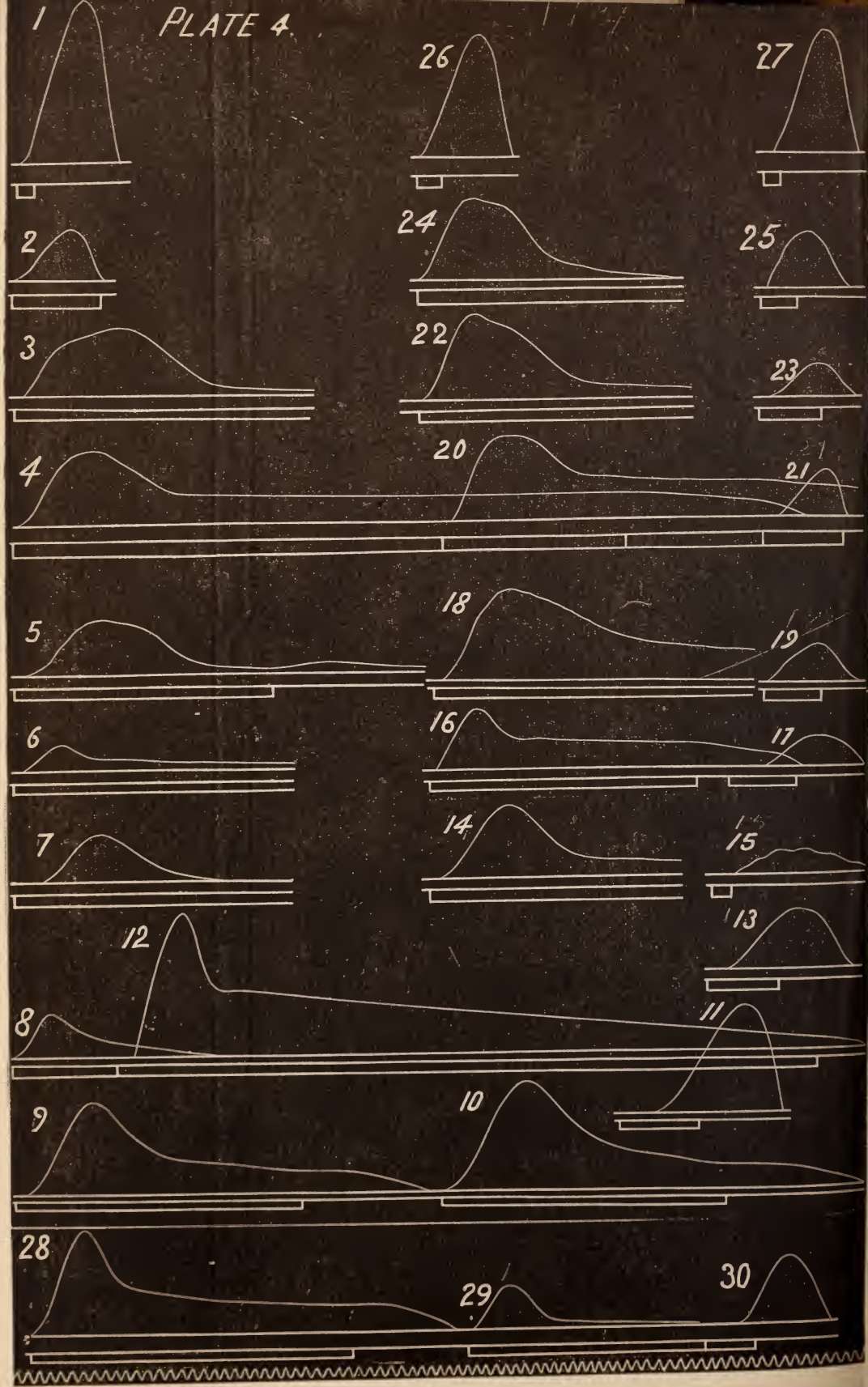


PLATE iv.—Myograms produced by direct stimulation of frog's muscles before and after section of the sciatic nerves.

FIG. 1.—Normal cacc 1 cell; fig. 26, normal ancc 1 cell; fig. 27, fc, with secondary coil 4. c. distant.

FIG. 2.—2d day of section cacc, 1 cell; fig. 24 normal ancc, 1 cell; fig. 25 fc, with secondary coil 4. c. distant.

FIG. 3.—3d day of section cacc, 1 cell; fig. 22 normal ancc, 1 cell; fig. 23 fc, with secondary coil 4. c. distant.

FIG. 4.—4th day of section cacc, 1 cell; fig. 20 normal ancc, 1 cell; fig. 21 fc, with secondary coil 4. c. distant.

FIG. 5.—5th day of section cacc, 1 cell; fig. 18 normal ancc, 1 cell; fig. 19 fc, with secondary coil 4. c. distant.

FIG. 6.—7th day of section cacc, 1 cell; fig. 16 normal ancc, 1 cell; fig. 17 fc, with secondary coil 4. c. distant.

FIG. 7.—16th day of section cacc, 5 cells; fig. 14 normal ancc, 1 cell; fig. 15 fc, with secondary coil in contact.

FIG. 8.—20th day of section cacc, 1 cell; fig. 12 normal ancc, 1 cell; fig. 13 fc, with secondary coil in contact.

FIG. 9.—37th day of section cacc, 5 cells; fig. 10 normal ancc, 1 cell; fig. 11 fc, with secondary coil in contact.

FIG. 28.—44th day of section cacc, 5 cells; fig. 29 normal ancc, 1 cell; fig. 30 fc, with secondary coil in contact.

Line at bottom marking $\frac{1}{100}$ ths of a second.

First, a curve was produced by cacc, then by ancc, then by an induced current from Dubois' secondary coil 4 centimetres distant. Forty-eight hours after section there was already diminished amplitude of muscular movements, shown by lower curves, and already $\text{ancc} > \text{cacc}$, while there was a commencement of tetanization in the anode curve, *i. e.*, the down-stroke was slanting and prolonged (pl. iv, fig. 24). On the third day ancc was $> \text{cacc}$, while fc was very slight indeed; cacc also showed tetanization, the curve being rounded and prolonged long after the electric stimulus was withdrawn. The latent period¹ was already lengthened (the normal duration of the latent period being about $\frac{1}{100}$ second). In certain pathological states² this period is much prolonged, and the time elapsing between the entrance of the electricity into the muscle and its contraction, is visible to the eye, but much better by aid of a myogram. This retardation is particularly marked in muscles showing degeneration reaction.

On the fourth day tetanization was still more complete, the muscle not relaxing at all during the continuance of the current, and not even till several hundredths of a second after the opening of the circuit.

On the sixth day cacc is smaller, and there appears also a small caoc ; ancc is larger and very tetanic.

On the seventh day cacc is small ($\frac{1}{6}$ the amplitude of the normal) and tetanic; ancc is $2\frac{1}{2}$ times $> \text{cacc}$.

On the sixteenth day, pl. iv, figs. 7, 14, 15, there is hardly a trace of cacc with one cell, while five cells produced a slow contraction retarded $\frac{5}{100}$ second, while the single cell still produced a large and tetanized ancc , *i. e.*, ancc was ten times $> \text{cacc}$. Fc was only obtained by bringing the second-

¹ The latent period is the time elapsing from the excitation of the muscle to its contraction.

² Mendelssohn: Sur le temp perdu des muscles. *Physiol. experiment.* (Marey), Paris, 1880.

ary coil of Dubois in contact with the primary coil, producing a current which threw the frog into general tetanus, the current thus produced being almost painful to the operator's hand.

On the twentieth day there was a small cacc (pl. iv, fig. 8) to one cell, while the anode of the same cell excited an immense tetanized curve. A fair fc, which is retarded, was produced by the secondary coil at contact.

On the thirty-seventh day the myographic curves presented peculiarities which, as examples to follow will show, must be considered as pathognomonic of the degeneration of nerves. From the first it has been shown that the down-stroke was getting more and more slanting and slow to reach the base line; *i. e.*, the muscle was slow to relax, and partly tetanized. Now, on the thirty-seventh day, and later, tetanization becomes complete, *i. e.*, so long as the galvanic current is allowed to pass through the muscle, it will not relax, but as soon as the circuit is broken, a slow relaxation takes place. This is shown in figs. 9, 10, and 28, pl. iv, where we have a sloping up-stroke, a rounded top, a sloping down-stroke to a certain point where it remains stationary until, when the circuit is broken, it gradually falls to the base line.

This series of myograms shows graphically the progressive changes, quantitative and qualitative, which an irreparable injury to a motor nerve produces in the tributary muscles. First, the retarded, tetanized, and gradually vanishing cacc; secondly, the retention of ancc, which, however, is also retarded and tetanized; thirdly, the preponderance of ancc over cacc; and lastly, the rapid failure of faradic contractility;—the later seemingly large contractions to faradism being the results of the application of a stimulus ten or more times the strength of the current first used. The reason that fc is retained at all is because the elec-

trode comes in direct contact with the muscle, the same thing occurring in man if the muscle be punctured. The only histological change noticed in the muscles of the frogs experimented on, was a granular, disintegrated condition of the intramuscular nerves.

The myographic studies undertaken in man¹ were chiefly in cases of nerve injury, or diseases which cause nerve-degeneration and muscular atrophy. The same appliances for registering were used in man as in frogs, but in a way the accompanying diagram will illustrate.

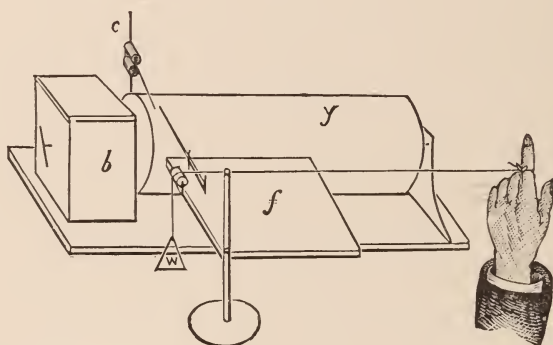


FIG. I.

MODIFIED MYOGRAPH FOR EXPERIMENTATION ON MAN.

- b.*—Clock-work with revolving cylinder, *y*.
c.—Chronograph apparatus (coils and needle).
f.—Frog-plate of cork, with its pulley and weight (*w*), and its writing lever, to which the movable part to be experimented on is attached by an inextensible thread.

The manner of procedure, suppose we be experimenting on the abductor indicis, is as follows. Place the hand on a rest of some kind at the same level as the frog-plate. Fix one electrode on an indifferent part of the body (preferably the sternum). Bind another small electrode over the ulnar nerve at the elbow or wrist, connect one directly with the battery, and the other with the battery through the mediation of the double interruptor. Turn on enough cells to

¹ Mention of human myography can be found in Cyon's "*Principes D'Électrothérapie*," and in some of Marcy's books, but the author has nowhere seen myograms from man, and but few from *any* mammal.

produce good *cacc*, set the cylinder going, and register several curves. Mark them *cacc*, and the number of cells used. Reverse the current, and register *ancc* the same way. Remove the cords from the galvanic battery, and connect with Dubois' secondary coil, and register *fc* the same way, noting the relations of the two coils (strength of current).

To take muscular reactions, leave the indifferent electrode on the same part, and bind or hold a small electrode on the motor point of the muscle to be tested.

In pl. ii, figs. 1, 16, 17, are seen healthy human myograms, produced by excitation of nerves, and the attention of the reader is particularly called to the similarity between them and those of the frog, the only difference being that in this subject the *ancc* could not be brought out on account of the great pain caused, and the excitation of antagonistic muscles. In speaking of pathological myograms the reader's time will be economized by giving the *name* of the disease studied (only typical, indubitable cases being employed), and other such data as are essential to a correct understanding of the subject.

In Bell's palsy of the face we have a disease very closely resembling nerve section, a resemblance which, in incurable cases, amounts to an identity as regards pathological changes and external phenomena. It matters not how muscles are severed from their centres, whether by neuroma, neuritis, compression, contusion, or section, an identical result will follow an irreparable injury, viz. : degeneration of the nerve and muscular atrophy. For this reason the accompanying case of Bell's palsy is a good one to place alongside of our frogs whose nerves were cut.

The mode of recording facial muscular movements was by means of connecting the cheek, in the neighborhood of the angle of the mouth, with the myograph, by means of a piece of isinglass plaster, stuck to the skin, from which a

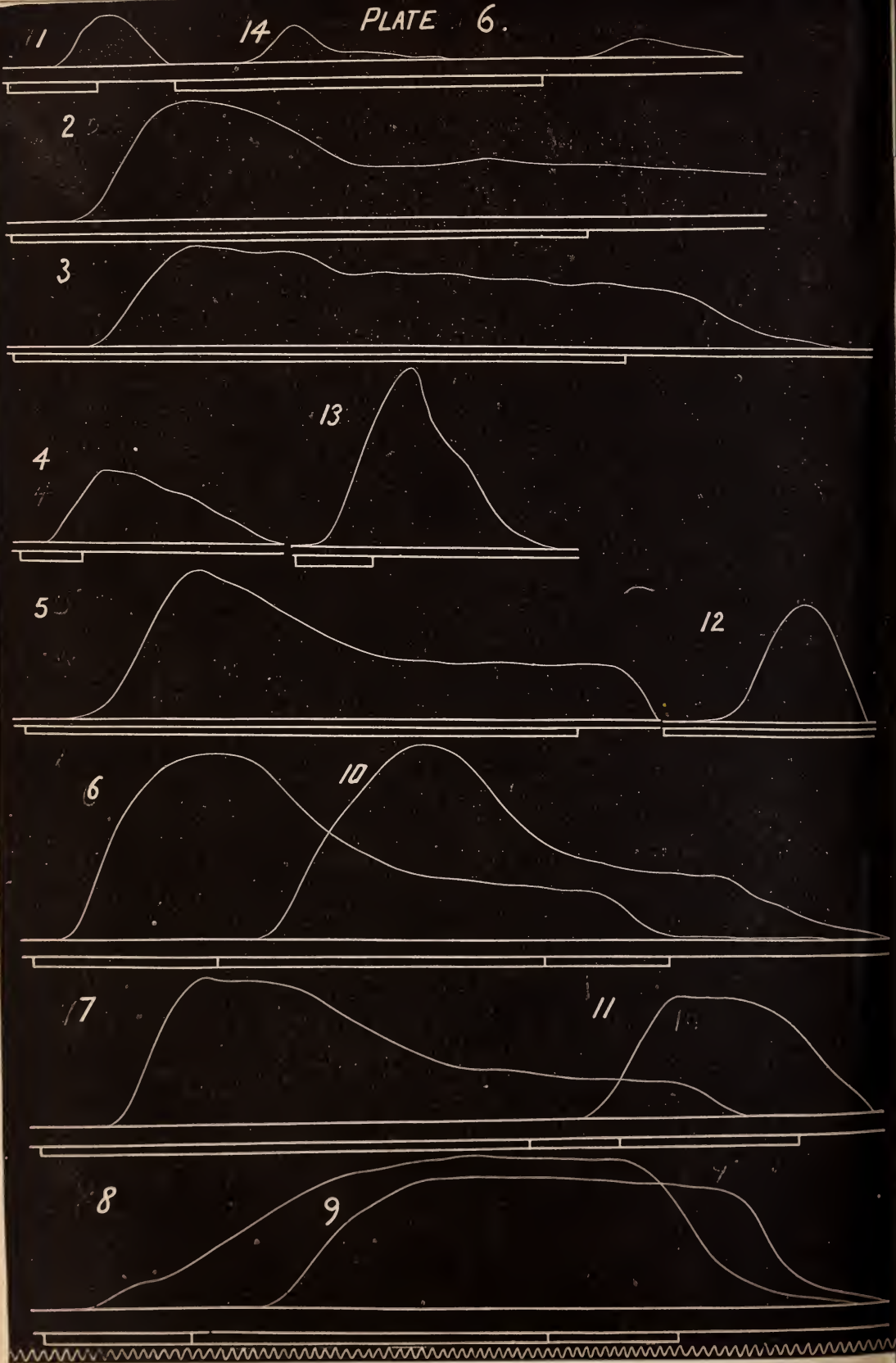


PLATE vi.—Human myograms, reduced one third. Case of Bell's palsy, etc.

FIG. 1.—Cacc 32 cells, and fig. 14, ancc and anoc 32 cells, nerve current, 14th day.

FIG. 4.—Cacc 25 cells, and fig. 13, ancc 25 cells, muscle current, 14th day. Ancc > cacc.

FIG. 2.—Cacc 40 cells, and fig. 3, ancc 35 cells; nerve current, 31st day.

FIG. 5.—Cacc 20 cells, and fig. 12, ancc 12 cells; muscle current, 31st day. (Ancc 12 cells very much > cacc 20 cells.)

FIG. 6.—Curve produced by extensors of the hand in a case of paralysis, produced by a blow on the musculo-spiral nerve, twenty-six days before. No nerve reaction, cacc 17 cells; fig. 10, ancc 40 cells, cacc > ancc; prognosis good.

FIG. 7.—Another case of paralysis of the musculo-spiral nerve from the kick of a horse. No nerve reactions, 60th day, cacc 40 cells; fig. 11, ancc 40 cells, cacc > ancc.

FIG. 8.—Cacc 47 cells, and fig. 9, ancc 40 cells, in a case of injury to the ulnar nerve by reducing a dislocated humerus with the heel in the axilla. Tracings from direct excitation of the abductor indicis muscle on the 60th day of the injury.

thread was carried to the lever. Then the head was placed so that, the thread being taut, any slight facial movement was recorded on the smoked paper.

The first examination was made in the second week of the paralysis. The formula then stood :

Nerve, cacc > anoc. No fc, nerve or muscle.
Muscle, ancc > cacc.

All contractions were much retarded and prolonged, the latent period of ancc and oc being about $\frac{7}{100}$ second. (See pl. vi, fig. 14.)

On the thirty-first day a second examination was made, when degeneration reaction was much more marked. The nerve was still excitable to galvanism however. The formula stood :

Nerve, ancc 35 cells about = cacc 40 cells ;
Muscle, ancc 12 cells about = cacc 20 cells,

thus showing a very well-marked degeneration reaction. The latent period of cacc, nerve current, was $\frac{15}{100}$ second ; that of ancc, nerve current, about the same (pl. vi, figs. 2 and 3), while the latent period of direct muscular excitation was $\frac{10}{100}$ second. Please compare trace 5, pl. vi, cacc in Bell's palsy on the 31st day, with fig. 9, pl. iv, cacc of palsied frog on the 37th day. The similarity, not to say *identity*, in the two curves is striking. Is it unreasonable to suppose that much the same histological changes were present in both ? In injury to the musculo-spiral nerve we have a peripheral palsy practically identical to Bell's palsy and that of nerve section in the frog ; therefore compare trace 6, pl. vi, cacc 17 cells and 10, ancc 27 cells, myograms in a case of musculo-spiral paralysis of the twenty-sixth day, with the trace of Bell's palsy on the 31st day (pl. vi, fig. 5), and the frog's myogram (pl. iv, fig. 9) on the thirty-seventh day. A second case of musculo-spiral

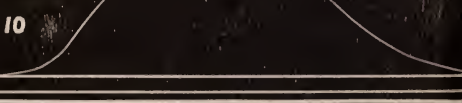
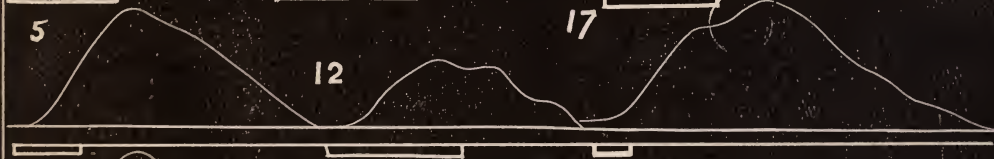
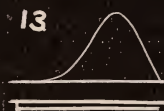
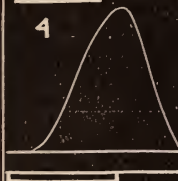
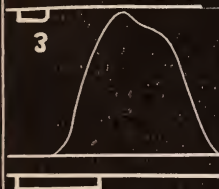
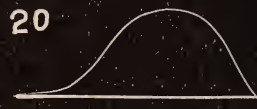
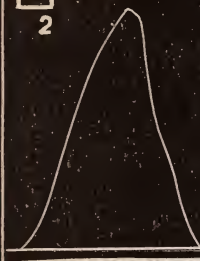
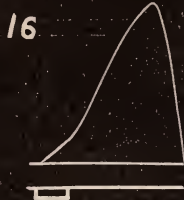
paralysis at the 60th day (pl. vi, fig. 7) may be added to the list, and a comparison of it with the four previously designated traces needs no word of comment.

There is a striking similarity in these myograms. All are *delayed*, have a *sloping* up-stroke, a *rounded* top, a *sloping* down-stroke to a certain point where the muscle remains *tonically contracted* till a *fall* occurs, about as soon after the *breaking* of the current as it commenced after the *making*.

There are, however, nervo-muscular disturbances in man which it is difficult, or impossible to reproduce in animals, and it will be in the briefest possible way that the myograms of various pathological muscles, chiefly the atrophic, are brought to the reader's notice. The reaction in these cases is almost invariably that of degeneration, *i. e.*, $\text{ancc} > \text{cacc}$, no fc and no nerve reaction, galvanic or faradic. In them also the reaction appears slow, worm-like, or wavy to the naked eye, which characteristics are magnified by the myograph.

Take, for instance, an old case of section of the median nerve, pl. i, figs. 6 and 11; a simple atrophy of the first interosseous muscles, pl. i, figs. 7 and 10; a case of functional atrophy of the first interosseous muscles from occupation, pl. v, figs. 6 and 7; an atrophy of the hand, non-progressive and with typical degeneration reaction, pl. ii, figs. 7 and 8. Take also the hand of chronic myelitis of the anterior horns in the adult, with an atrophy located in the first interosseous space, in this respect resembling the atrophy of progressive muscular atrophy, but in others entirely differing from it, chiefly by the fact that it has typical degeneration reaction, see pl. i, figs. 8 and 9, where ancc (25 cells) is 4-5 times $> \text{cacc}$ (30 cells). True degeneration reaction is also shown in traces 5 and 8, pl. v, where we have myograms of the abductor indicis, in a case of

PLATE 1



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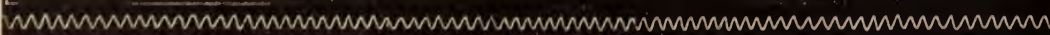
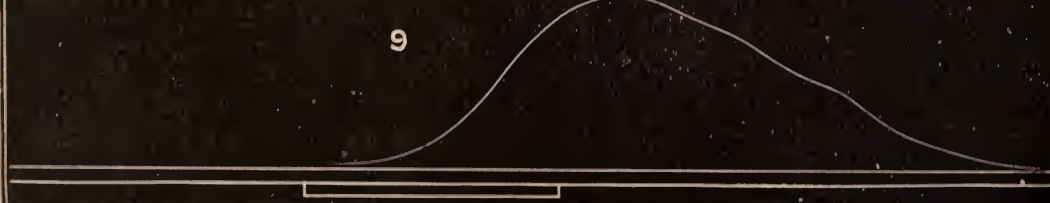


PLATE i.—Human myograms from direct muscular excitation, reduced one third.
Tuning-fork line showing $\frac{1}{8}$ ths of a second.

FIG. 1.—Cacc 47 cells; fig. 16, ancc 47 cells; fig. 21, fc + 2, of normal abductor indicis muscle.

FIG. 2.—Cacc 42 cells; fig. 15, ancc 47 cells; fig. 20, fc + 2, of abductor indicis muscle in a case of long disuse of hand from rheumatism.

FIG. 3.—Cacc 28 cells; fig. 14, ancc 40 cells; fig. 19, fc + 4, of anterior tibial muscles in a case of progressive muscular atrophy.

FIG. 4.—Cacc 35 cells; fig. 13, ancc 35 cells; fig. 18, fc + 2.5, of abductor indicis in a case of partial recovery from myelitis of the anterior horns in the adult.

FIG. 5.—Cacc 30 cells; fig. 12, ancc 30 cells; fig. 17, fc + 2, of calf-muscles in a case of pseudo-hypertrophic paralysis.

FIG. 6.—Cacc 47 cells; fig. 11, ancc 47 cells, of opponens pollicis in a case of old section of the median nerve.

FIG. 7.—Cacc 47 cells; fig. 10, ancc 47 cells, of abductor indicis in a case of simple atrophy of the first interosseous space.

FIG. 8.—Cacc 30 cells; fig. 9, ancc 25 cells, of the abductor indicis in a case of chronic myelitis of the anterior horns simulating commencing progressive muscular atrophy in the distribution of the wasting.

lepra anæsthetica with neuroma on each ulnar nerve, and with anæsthesia and atrophy in their distribution. The ancc curve (5) is identical with the tetanoid curves compared on the page 16.

There are some changes which muscles undergo by which their electrical reactions are changed, but not much so, and hardly enough to be visible to the unaided eye. These changes are principally quantitative, not qualitative.

Let us see how much our graphic method will help us in this matter.

The immobility consequent on long disuse of a part, as by retention of a member on a splint for a fracture or some inflammatory trouble, as rheumatism, every one has seen and undoubtedly has thought it more than can be accounted for by the false ankylosis which is often present. Persons acquainted with the use of electricity have also noticed that the nerve and muscle reactions in these cases are slow, but not qualitatively changed. Myograms 2, 14, 15, pl. ii, and 2, 15, 20, pl. i, show the changes. The muscular contraction is delayed and slightly tetanized. All authorities agree that there is no true degeneration reaction in progressive muscular atrophy, but this fact is better demonstrated by consulting pl. ii, 3, 12, 13, and pl. i, 3, 14, 19.

In pseudo-hypertrophic paralysis the normal formula is retained, but the myograms are peculiar, pl. i, 5, 12, 17, and pl. ii, 4, 10, 11. A word before closing as to the reactions in a peculiar case of paraplegia occurring in a girl who had recovered from a specific paralysis of the sixth and seventh nerve on the same side. The paraplegia was simple (no bladder or rectal trouble), and was almost confined to paresis of the anterior tibial and peroneal muscles. The reactions are interesting because they resemble degeneration reaction to galvanism as put down in the books, but faradic contractility is still retained.

The traces were made by excitation of the peroneal nerve and the anterior tibial group of muscles.

The nerve reactions were :

$$\left\{ \begin{array}{l} \text{anoc 15 cells,} \\ \text{pl. v, fig. 13,} \end{array} \right\} > \left\{ \begin{array}{l} \text{ancc 15 cells,} \\ \text{fig. 2,} \end{array} \right\} > \left\{ \begin{array}{l} \text{caoc 20 cells,} \\ \text{fig. 14,} \end{array} \right\} > \left\{ \begin{array}{l} \text{cacc 20 cells,} \\ \text{fig. 1 ;} \end{array} \right\}$$

an almost complete reversal of the formula.

The muscle reactions were :

$$\left\{ \begin{array}{l} \text{caoc 10 cells,} \\ \text{pl. v, fig. 11,} \end{array} \right\} \text{ almost } = \left\{ \begin{array}{l} \text{cacc 10 cells,} \\ \text{fig. 3,} \end{array} \right\} > \left\{ \begin{array}{l} \text{ancc 21} \\ \text{cells, fig. 4,} \end{array} \right\} > \left\{ \begin{array}{l} \text{anoc 30 cells,} \\ \text{fig. 9.} \end{array} \right\}$$

There was delayed and slow fc in nerve and muscle, which of course precluded the possibility of a destruction of the anterior horns of the spinal cord, as the paraplegia was of five months' standing.

A careful perusal of the explanatory text accompanying the plates and a comparison of the different traces among them will repay the reader by noticing:

First, the identity as to form and relation of human and frog myograms in health.

Secondly, the great similarity of the pathological myograms in frogs and man (section or disease of nerves).

Thirdly, the marked myographic changes where slight trophic disturbances affect human muscles (disuse, muscular atrophy, etc.).

Fourthly, the profound alterations in contour of myograms of muscles for a long time severed from the vitalizing power of their nerve centres (chr. musc. atrophy, chr. poliomyelitis, etc.).

The future of human myography cannot be predicted. It may become useful as a very accurate mode of differential diagnosis and prognosis. As an adjunct to our labora-

PLATE 2

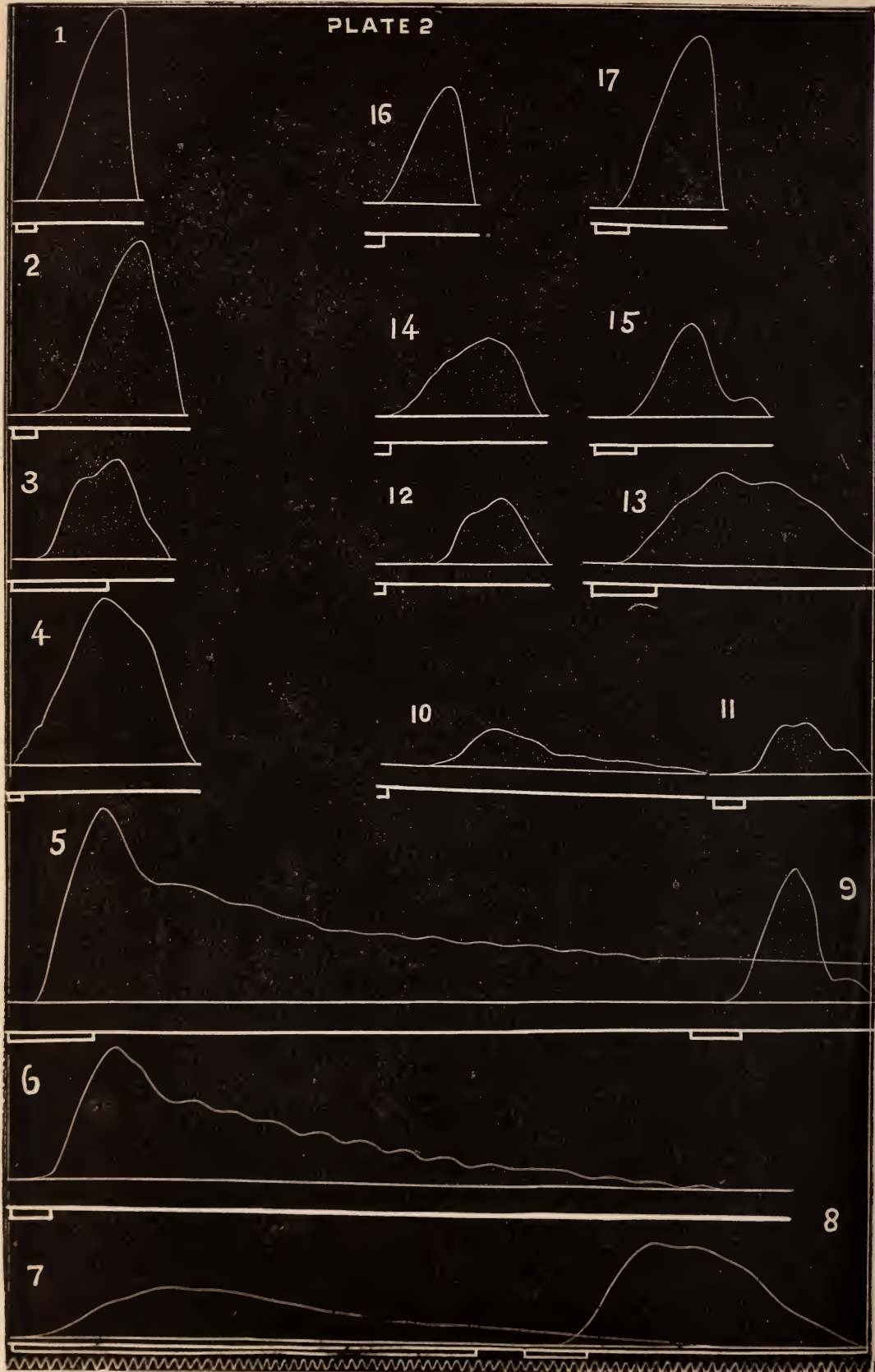


PLATE II.—Human myograms, all but last two produced by excitation of nerves; reduced one third. Tuning-fork mark at the bottom = $\frac{1}{8}$ second.

FIG. 1.—Contraction of normal abductor indicis, cacc 32 cells; 16 = anoc 40 cells; 17 = fc contact.*

FIG. 2.—Case of long disuse of hand from rheumatism and splint; abductor indicis. 2 = cacc 27 cells; 14 = anoc 42 cells; 15 = fc + 2.5.

FIG. 3.—Case of progressive muscular atrophy. Myograms of the anterior tibial muscles. 3 = cacc 28 cells; 12 = anoc 34 cells; 13 = fc + 4.

FIG. 4.—Case of pseudo-hypertrophic paralysis. Myograms of calf-muscles. 4 = cacc 25 cells; 10 = anoc 25 cells; 11 = fc + 2.

FIG. 5.—Old hemiplegia with contracture. Myograms of abductor indicis. 5 = cacc 35 cells; 6 = anoc 40 cells; 9 = fc. + 5.

FIG. 7.—Case of atrophy of first interosseous space, not very progressive. Degeneration reaction. Myograms of abductor indicis. 7 = cacc 47 cells; 8 = anoc res. fc. These last two traces were taken by direct excitation of the muscle, there being no nerve reaction.

* Here and elsewhere contact means Dubois' secondary coil touching the primary. + 2 means the secondary coil overlapping the primary 2 centimetres. — 2 means the secondary coil being distant from primary, 2 centimetres.

tories for the study of experimental therapeutics it needs no lauding. Its employment is far more simple and occupies much less time than one would think, and the American apparatus is so much cheaper than the imported, that it comes within the reach of almost any one.

PLATE v.—Pathological human myograms. Nerve reactions in case of specific paraplegia, etc.

FIG. 1.—Cacc 20 cells; fig. 14, caoc 20 cells.

FIG. 2.—Ancc 15 cells; fig. 12, fc; fig. 13, anoc 15 cells, same case.

FIG. 3.—Cacc 10 cells; fig. 11, caoc, 11 cells, same case; from muscular excitation.

FIG. 4.—Ancc 21 cells; fig. 9, caoc, 30 cells; fig. 10, fc, same case; from muscular excitation.

FIG. 5.—Ancc 35 cells; and fig. 8, cacc 47 cells, in case of lepra anæsthetica. Muscular excitation.

All the above curves reduced one third.

FIG. 6.—Cacc 47 cells; fig. 7, ancc 47 cells, in case of functional atrophy of the hand by direct excitation of the abductor indicis muscle.

NOTE.—For all the curves except 6 and 7, the tuning-fork line = $\frac{1}{28}$ ths of a second; for curves 6 and 7 it denotes $\frac{1}{100}$ ths of a second.

LEAD PARALYSIS.*

By S. G. WEBBER, M.D.,

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MY object in presenting a few cases of lead paralysis is not to discuss all the questions which naturally occur to one in connection with the subject. I shall limit myself to a brief review of the theories of its pathogenesis, and secondly to calling attention to some unusual forms in which lead poisoning may show itself, simulating more serious lesions.

There are two views, now most prevalent, in regard to the seat of the lesion in lead paralysis. Some authors consider the central nervous system, the spinal cord and brain, to be the parts primarily affected. Erb, E. Remak, Eulenberg, Monakow, Bernhardt, de Watteville, favor this theory; Westphal, Leyden, Zunker, Gombault, Charcot, favor another view—that the primary seat of disease is in the nerves.

The earliest case, with autopsy and examination of the cord, of which I have found report is one by Lancereaux. (*Gaz. méd. de Paris*, 1862, p. 709.)

The patient was 37 years old, had suffered from lead colic at 14 years of age, had been paralyzed in forearms and legs for 10 years before death. At the autopsy the brain and medulla ob-

* Read at the meeting of the American Neurological Association, June 23, 1882.

longata were found normal. Just above the cervical enlargement at the level of the second and third cervical vertebræ the spinal pia mater was discolored over an extent of four or five centimetres. Here the cord was small and seemed atrophied and slightly softened. Some of the motor nerve roots at this level were smaller than those either above or below. Just above the lumbar enlargement was a similar spot of softening, but less marked, and some of the anterior and posterior roots seemed atrophied. On right side only two nerve roots in the upper part of the brachial enlargement were atrophied, and in these were healthy nerve fibres mixed with some which were altered, showing fine fat granules in the place of the medullary sheaths. The nerve trunks, going to the muscles, which had suffered most change, were much more diseased than the nerve roots, some of the fibres being reduced to mere connective tissue.

The cord presented what appeared to Lancereaux insignificant changes—only some cells were more granular than normal, deformed and atrophied.

The muscles showed degenerated fibres mixed with those less altered, retaining their striations.

In a second case which he reports, where the paralysis had been of four years' duration, the nerves supplying the affected muscles were much affected, their myeline being finely granular. The cord was normal in structure, but showed a congenital anomaly of development; the nerve cells, however, being healthy. Many of the fibres of the affected muscles were reduced to the sarcolemma with nuclei enclosed therein. (Lancereaux, *Saturnisme chronique avec accès de goutte et arthrites uratiques. Gaz. méd. de Paris*, 1871, p. 385.)

C. Friedländer (Anatomische Untersuch. eines Falles von Bleilähmung nebst Begründung der myopathischen Natur dieses Affection. *Virch. Arch.*, Bd. 75, 1879, p. 24) found the nerves degenerated within the muscles, also in the nerve trunks and even in their roots. There was no change in the spinal cord. The muscles had also undergone degeneration; the muscular nuclei remained.

Friedländer considered that the change begins in the muscles, and that the nerves are affected only secondarily.

Gombault (*Contribution à histoire anatomique de l'atrophie musculaire saturnine. Arch. de Physiol.*, v, 1873, p. 592) found the spinal cord and nerve roots healthy. The peripheral nerves were much altered. The nerve fibres were reduced in size; many contained granular myeline, and some sheaths were folded upon themselves, enclosing series of ovoid nuclei. The interstitial tissue was increased, and its nuclei were also multiplied in number. This was especially the case around the blood-vessels.

He comments upon: 1. The generalization of the lesion, spreading to a considerable number of muscles, after beginning in the posterior part of the forearm, where it reached its greatest intensity. 2. The so-to-say systematic distribution of the lesion in the muscles, explained by the neuritis of corresponding nerve trunks. 3. The perfect symmetry in the absence of any alteration of the central nervous system. 4. The nature of the process, which consists essentially in a proliferation of the interstitial tissue, while the proper elements of the organ degenerate and are atrophied. He refers to a similar change in the kidneys.

F. Tiburtius (*Die Extensorenlähmung bei chronischer Bleivergiftung. Inaug. Dis., Zurich, 1876.* Only quotations from this have been seen) found the most important changes in the intramuscular fibres, which were greatly atrophied. The same change, less intense, was found in the musculo-spiral nerve, which was surrounded by connective tissue rich in nuclei; the interstitial connective tissue was not increased. The spinal cord and brain were normal. The patient had the usual form of paralysis, and also suffered from epilepsy.

Eisenlohr (*Idiopath. subacute Muskellähmung und Atrophie. Cbl. f. Nervenheilk.*, 1879, p. 100) found the muscles

much changed ; also marked atrophic change in the intramuscular nerves and the nerve root, the roots less than the muscular nerves. The cord was completely intact.

C. Westphal (Ueber eine Veränderung des Nervus radialis bei Bleilähmung. *Arch. f. Psych.*, etc., iv, 1874, p. 776) reports in regard to the changes found in Bernhardt's patient. The radial nerve showed a diminution in the number of normal nerve fibres without increase of connective tissue. The place of the nerve fibres, which had disappeared, was occupied by groups of small fibres without axis-cylinders. He considered these to be nerve fibres in process of regeneration. In the cord and nerve roots he found no change from the normal structure. Subsequently (vol. vi, 1876, p. 802) he says he thinks the above condition of the nerve fibres is less proof of the pathological change than when he examined the nerve, as he found a similar condition in normal nerves, but never in such excess as in the lead case.

Charcot (*Compt. rend. de la Soc. de Biol.*, 1879, p. 46) says he has several times examined the spinal cord in cases of lead paralysis ; has never found clear lesions as are found in spinal amyotrophy.

Déjérine (Recherches sur les lésions du système nerveux dans la paralysie saturnine. *Mem. de la Soc. de Biol.*, 1879, p. 11) examined the nerves and muscles in five cases of lead paralysis. He found the muscular nerves altered in all five ; he also examined the radial nerves at different heights, and found the change in that nerve ; in one case he could examine the radial at the point where it took its origin from the brachial plexus, and recognized the change there. He then examined all the anterior roots of the cervical region in four cases, and only some of the roots in the fifth case. In two cases the anterior roots presented changes similar to those found in the peripheral nerves, excepting that it was less intense ; the nerve fibres were changed, and in some

cases the axis-cylinders had disappeared; the connective tissue was increased. This hyperplasia of connective tissue was not regular in its distribution. In the other three cases there was no such change discoverable in the nerve roots.

Déjérine does not give with this memoir the examination of the spinal cords, but rather promises that at some future time. In the discussion which followed his paper (*Compt. Rend. de la Soc. de Biol.*, 1879, p. 46), he said that he had not found any characteristic lesions in the cord, only some pigmented cells.

Although his examination, as given, includes only the nerves he does not hesitate to state his belief that the primary lesion is to be sought in the spinal cord.

In six cases of which we have full reports the spinal cord was found healthy; Charcot, besides, says in a less definite way that he has several times found the cord healthy. In Lancereaux's first case, the cord was somewhat diseased, but apparently not enough so in his opinion to cause all the disturbance in the muscles and nerves. In all the above cases the nerves which supplied the affected muscles were diseased, and in some of these the proximal parts are said to have been less affected than the more distal. Déjérine does not give his examination of the spinal cords in his five cases in detail, but says in a general way that he had not found any characteristic lesions in the cord; and though this was said in the discussion which followed the reading of his paper, it would not be unreasonable to believe that he referred to his five cases. He found, like the others, the nerves near or within the muscles most diseased; the nerve roots were not diseased in three of his cases, and only slightly diseased in two.

I. Moritz (*Journal of Anatomy and Physiology*, Oct., 1880, p. 78. A contribution to the pathological anatomy of lead paralysis) was unfortunately able to examine only

the nerves and muscles; he could not examine the nerve roots nor the spinal cord. The intramuscular nerve fibres showed considerable thickening of connective tissue and multiplication of nuclei; in some fibres the axis-cylinders could no longer be seen. "The intramuscular nerve fibres were more uniformly and more extensively affected" than the muscles, "and hence it is probable that the process commenced in them and that the myotic changes were merely secondary."

The value of this observation is slight, but as far as it goes it favors the view that the nerves are primarily affected.

The evidence obtained by these observers from *post-mortem* examinations would be conclusive that lead paralysis does not depend upon lesion of the cord; but objection has been raised that it requires only a very limited lesion in the cord to give rise to the symptoms observed, and it is said that the examinations have not been complete. Remak (*Zur Pathogenese der Bleilähmungen, Arch. f. Psych. u. Nervenk.*, vi, 1876, p. 1) located the origin of the nerves supplying the extensors of the forearm above the cervical enlargement, and suggested that the examination had not been directed to that portion of the cord. He believes the lesion is seated in the cord, chiefly because one group of muscles which naturally act together are affected to the exclusion usually of another muscle, the supinator, which, also supplied by the radial nerve, acts rather with the biceps, and is probably supplied by fibres coming from another level of the cord. He also derives argument for his view from other forms of paralysis of spinal origin, as infantile paralysis, where groups of muscles acting together are affected to the exclusion of other muscles lying near but not acting with them.

Three years later, in the same journal (vol. 9, 1879, p. 572), he withdrew part of what he said as to the origin of the

nerves going to the extensors, and said it is more probable that the nuclear origin of these nerves lies in the cervical enlargement itself, and, indeed, near its middle section, and here is where the anatomical changes in ordinary forms of lead paralysis are to be sought (p. 574). This is the locality which has been carefully examined by several observers and nothing abnormal found.

Four observers have found the cord diseased. One of these, Zunker (*Zeitschr. kl. Med.*, i, 1880, p. 496), does not refer the symptoms to the cord as its primary seat, but thinks the nerve is first affected; that is, that the paralysis from lead poisoning is owing to a neuritis. He found the nerves in their smaller divisions greatly changed, and this change decreased upward, and was not found in the nerve roots near the cord.

He also found disease of the cord, an irregularly distributed atrophy of the gray substance; the large ganglion cells of the anterior cornua were diminished in number and size; in spots in the lower dorsal region there was complete disappearance of these cells.

Another, Vulpian ("Maladies de la Moelle," 1879, p. 158), considers that the primary change is in the cord, and that the paralysis in lead poisoning depends upon a myelitis. He says he has seen alterations in the spinal cord in a case of lead poisoning. Some nerve cells contained vitreous, colloid masses; others were undergoing atrophy; there was perhaps a little multiplication of nuclei. In this case the peripheral nerves and the muscles were altered. He also found islets of sclerosis in the roots of the nerves of the cervical enlargement.

In a dog poisoned with carbonate of lead he recognized a clearly marked myelitis. In several regions of the cord the nerve cells were undergoing destruction; the nerve fibres were altered.

Birdsall (A contribution to the pathological anatomy of lead paralysis. *The American Journal of Neurology and Psychiatry*, May, 1882, p. 176) found slight changes in the anterior cornua in the cervical region. The peripheral nerves were not examined, but no decided degeneration was found in the anterior roots. Birdsall refers also to an unpublished case of Seguin's where there had been partial recovery from lead paralysis; slight changes were found in the nerves and muscles, and there was a slight reduction in the size and number of the cells of the anterior cornua in the cervical region. Both these cases as given by Birdsall are simply of value as showing that changes occur in the cord, but they do not aid in deciding where the pathological process commences.

C. V. Monakow (Zur pathologischen Anatomie der Bleilähmung und der saturninen Encephalopathie. *Arch. f. Psych. u. Nervenk.*, Bd. x, 1880, p. 495) reports a case which is of more than usual interest and is very significant. He found lesions in the brain, the cord, and the nerves. The location of the lesions was such that the changes in the cord could not be secondary to the changes found in the brain, and, as the distal ends of the nerves were more affected than the proximal portions, the roots were not diseased, it is not probable that the changes in the nerves depended upon those found in the spinal cord.

He says, in reviewing the changes found in the cord and nerves: "If the pathological changes in the extensors, the radial nerve, and the right anterior cornu are compared with each other, there can be no doubt that a close connection exists between them. The principal change in the spinal cord was in the vicinity of the exits of the sixth, seventh, and eighth cervical nerves, at just the points whence arise the roots of the radial nerve, innervating the extensors, and since there were no other symptoms of motor

disturbance in the affected arm, one must consider this spot in the middle nucleus as connected with the paralysis of the extensors. But another question is: Whence did the paralysis arise—from the muscles, the trunk of the radial, or from the spinal cord?" He concludes that the origin of the affection is to be sought in the spinal cord, though recognizing that there are difficulties in this view from the absence of changes in the radial nerve near the cord, and in the nerve roots.

Erb (Ein Fall von Bleilähmung, *Arch. f. Psych.*, etc., v, 1875, p. 445) reports a case without autopsy, and concludes that we must look upon lead paralysis as most probably central, or at least of neuropathic origin, although the central lesion cannot yet be proved by any examinations which have as yet been made.

In the nervous system there have been found, then, changes in the motor centres in the brain (Monakow), atrophy of the ganglion cells, thickening of the walls of the vessels, and increase of the neuroglia; in the cord were found changes independent of all cerebral changes: either the brain not being diseased, or, as in Monakow's case, there being none of the usual secondary degenerations connecting the lesions in the brain with that in the cord. In the nerves there have been lesions which were at a distance from the cord, and which could not be seen to have any anatomical connection therewith. Either, then, these lesions of the motor tracks and centres are connected in ways not yet discovered, or the lesion originates independently in each of the three places.

The fact that it is the motor tract which is generally most affected, though not exclusively, would favor the view that there is a relation between the different centres of change.

We have as yet no proof that the higher centres, as brain or cord, can cause lesion of distant parts of the nervous sys-

tem, as cord or nerves, without a continuous tract of degenerative tissue intervening. Such a relation may in the future be shown to exist, but at present it is not proven. On the other hand there are a few facts, as experiments on animals and the changes in tetanus and hydrophobia, which tend to show that the peripheral nerves may exert an injurious influence upon the nutrition of the cord, even when the intervening part of the nerve is apparently unaltered. In lead paralysis the mass of evidence derived from pathological anatomy would show that the disease is either primarily a neuritis, or the origin by separate and independent centres may be the true explanation. There are several considerations which would justify the latter conclusion. In Monakow's case and in some others there were changes in different parts of the nervous system which seemed to have no connection one with the other. Not only have there been found interstitial changes in the nervous system; the same kind of changes has been found in the kidneys (Gombault), and the bones have been found diseased (Lancereaux). The sympathetic ganglia have also been found affected with this interstitial degeneration. Probably, therefore, lead poisoning is favorable to the development of interstitial changes; when this influence is exerted upon the nervous system, we have symptoms which vary according to the seat of the change: cephalic symptoms when the brain is affected; spinal-paralysis, tremor, atrophy, according to location, when the disease is in the spinal-cord; and those which arise from neuritis when the nerves are affected; or a combination of symptoms when more than one portion suffers.

The fact that many cases of lead paralysis recover is in favor of the peripheral rather than central origin of the disease. Some years ago a worker in a white lead manufactory was under my care with extreme atrophy of the ex-

tensors of both arms; the muscles were, as it were, dissected out, leaving a cavity where they had been. There was entire loss of reaction to the faradic current, and the muscles responded very feebly to the galvanic current. After a long course of treatment he regained partial use of the muscles. Two years after, the muscles had recovered their normal size, responded perfectly to the will, and were of normal strength, only one muscle on the right gave a less than normally ready response to the faradic current. We cannot believe that the spinal cord is the primary source of the atrophy in this and similar cases. Most of the examinations of nerves and muscles in lead paralysis show that their nuclei still persist, long after paralysis has shown itself; from these nuclei may be developed the new structures which take their part finally in restoring the normal functions of the limbs. The fact that so many observers find the spinal cord intact may encourage us, then, to give a relatively favorable prognosis, unless the changes are so extensive and have lasted so long as to lead to the opinion that the cord is diseased. This, however, is a question in diagnosis which it will often be difficult to answer definitely, and we shall have to wait for time and the result of treatment to show how serious the changes have been.

Another fact, which has not been referred to as much as I think it merits, and which will lend weight to the view that the paralysis is caused by a variety of neuritis, is found in the sensory symptoms. In every case in which I have questioned the patients, there has been either pain more or less severe, or soreness, or a tingling sensation preceding the motor disturbance.

The first case which I report is one of undoubted lead paralysis; the girl was brought in contact with lead in her employment, and probably used white lead as a cosmetic; there was the lead line on her gums. The attack seems,

from the patient's statement, to have developed in its last degree rather suddenly. Probably, if the patient had been more intelligent or less timid it would have been found that the loss of power in the legs was more gradual than the patient's account would indicate.

CASE I.—Susan D., æt. 18. One year ago began to have pains in limbs; was sick three months, after which she was well till about a month ago; then she began to vomit, and had severe pain in stomach. Two weeks ago she lost the use of her legs and hands, and wrist dropped. Pain in the bowels has been less since the paralysis came on. A few days before the loss of motion there was a sense of numbness in soles of feet and legs, as if they were "asleep." The loss of power came on suddenly; she rose in night and could not stand; the next morning power of motion was gone. The loss of power in hands was more gradual.

In both arms paralysis of extensors and supinators; flexors and interossei still act. All the motions that are possible are executed very feebly and with considerable tremor. The faradic reaction is lost in the paralyzed muscles; retained, but diminished, in those over which she has partial voluntary control. Galvanic current, 25 cells move muscles on left; have no effect on right.

She could turn in bed, could draw legs up feebly, could perform some of the motions with feet, but feebly. Faradic reaction much diminished; 25 cells galvanic current move muscles of legs slightly.

Well-marked lead line on gums.

Urine contained no albumen; some hyaline casts.

Was treated with galvanism and iodide of potassium and iron. It was necessary to suspend the iodide occasionally on account of symptoms of acute lead poisoning, which arose during its use, but which subsided as soon as the medicine was omitted.

She stayed in the hospital six months, and when discharged was able to use her hands reasonably well; was able to walk fast; could not run.

Five months later she was in hospital again, still able to walk, right arm not quite so strong, troubled with colic. She improved, but would not stay more than about a month.

The second case was one in which syphilis might have been an element in the etiology, but lead being found in

the urine it was thought more probable that metal was the cause of the paralysis. The recovery was more complete than I have generally seen in cases of syphilitic disease of the cord. Subsequently the man was in the hospital again for Bright's disease, during which plaques muqueuses appeared.

CASE 2.—James H. S., æt. 28, was a hard drinker ; complained, on entrance, of pain in abdomen, constipation, and weakness of legs. When seen, the patient was in a dull mental state, as if recovering from a severe drunk. The legs could only be moved feebly ; there was entire absence of plantar reflex ; the cremasteric reflex persisted. Patella tendon reflex was absent. Sensation was preserved to a limited extent, but his condition was such as to render accurate examination impossible. There was œdema of lumbar region and of the legs. The hands and arms could be moved naturally.

Expiration was prolonged over the bases of both backs, vocal resonance was more clear on left than right, percussion resonance equal on the two sides. Murmur with first sound of heart at apex, area of dulness slightly increased. There was tenderness over right hypochondrium ; the hepatic dulness extended below the margin of the ribs.

A few days later he complained of numbness in his hands, and the strength was diminished in them, and finally he could not extend hand and fingers ; could close hand feebly. The cremaster reflex was then absent, and there was priapism, the penis being constantly semi-erected.

Respiration was natural, but he complained of shortness of breath.

Urine was without albumen and casts ; contained lead.

Pain in back led to the use of dry cups, which gave relief.

About seven weeks after entrance he began to regain freer motion of hands ; the right, which had suffered less than the left, could be fully extended, the left nearly so ; grasp was stronger. A month later he could stand and take a few steps without support. He finally recovered so as to be able to go back to his duties in the fire department.

The third case is of interest as showing the good result of persevering treatment ; and while in the hospital it was

interesting to compare her symptoms with those of another patient with myelitis, in whom the hands and legs were similarly affected, the muscles showing the same changes to electrical reaction, in whose urine no lead was discovered, who remained in the hospital nearly as long, but did not improve.

CASE 3.—Margaret F., æt. 27, married, entered Nov. 8, 1881 ; no children ; one miscarriage three years ago. Her mother died from paralysis, and her father from the effects of a bad cold. Two years ago she lost the use of both lower extremities, and was troubled a good deal by excessive weakness of the arms. She remained in bed thirteen months ; finally recovered completely.

About two weeks ago again began to have partial paralysis of legs and weakness of arms ; the loss of power has increased. Preceding the commencement of this paralysis she had headache and dizziness for about a week, and during that week noticed that her knees seemed to give out on going down stairs ; also had sharp pains in the calves of her legs ; there was also some numbness in feet and hands.

She could move her legs slightly in bed ; they were kept slightly flexed all the time ; she could not extend them entirely on account of discomfort about the knees. Reflex from irritation of the sole of feet was slightly delayed. Sensation to touch in the legs was slightly diminished. The extensors of both hands were paralyzed, the flexors weak ; there was no pain on passive motion of the hands ; the opponens pollicis acted. Motions of the shoulder and elbow were fairly strong ; there was a slight diminution of the sense of touch in the hands. The tongue protruded straight ; the pupils reacted normally ; there was no facial paralysis, no lead line on gums.

On the right the muscles of the forearm and hand responded to the faradic current ; on the left the extensors did not act ; the other muscles responded only feebly. In the legs the tibialis posticus acted feebly, the tibialis anticus not at all, to the faradic current. In the arms the galvanic reaction of the extensors was increased, as compared with the flexors.

Iodide of potassium was given, and after some days the urine examined for lead. A small amount of lead was found. Electricity, massage, and passive motions were used ; the iodide of potassium was continued. After about two months she began to

regain power in moving the hands ; the fingers and hands could be partially extended ; the right hand gained more and faster. Passive extension of legs and flexion of the feet caused considerable pain. She was discharged about the middle of March, 1882, able to walk with help. The faradic current did not cause contraction of any of the muscles of the legs, but the will had control over the muscles of the thigh, and she could move the toes ; the affected muscles responded with more than normal readiness to the galvanic current.

The fourth case was rather unusual in its grouping of symptoms ; in some respects resembling lateral sclerosis, there being stiffness, contraction, or rigidity of the limbs, and very greatly exaggerated patella tendon reflex.

CASE 4.—Mary J., married, æt. 23, was well, excepting for last two or three years she had headache occasionally ; three months ago began to have numbness in both feet and hands, gradually increasing, attended with rather constant pain running up knees and elbows ; legs were swollen. For three weeks she was unable to walk on account of stiffness of legs. No cramps nor twitching of legs ; they do not draw up in sleep ; catamenia regular ; other functions of body in normal action.

Both feet were entirely paralyzed, in extension ; attempt to flex feet caused pain in feet and under the knees. Sensation was delayed in the feet four or five seconds ; sensation of pain persisted after impinging body was removed. Above knees sensation was normal. Absence of plantar reflex. She could draw her legs up slowly and with considerable effort. Patella tendon reflex exaggerated.

In the upper extremities motion at the shoulders was moderately good ; at the elbow very weak ; extension of the wrist and fingers lost ; flexion of wrist and fingers feeble ; action of interossei nearly lost ; rotation of arm feeble.

Gums were shrunken away from the teeth ; there was no lead line.

The faradic reaction was diminished or lost in the muscles most affected. No separate note was kept of differences.

The urine was without albumen ; contained considerable lead after iodide of potassium had been given.

Galvanic current and massage were used. Iodide of potassium was given internally.

She was under treatment nearly five months, during which she regained partial power over the hands, and recovered use of legs so that she could walk about without help.

Excepting the last case I could give the histories of other patients under treatment at the same time with very similar symptoms, in whose urine after using iodide of potassium no lead was found, and who did not improve under treatment. It would extend this paper to too great a length, and would probably add nothing to its practical value.

Dr. F. Minot published some cases of lead poisoning in the *Boston Med. and Surgical Journal* for March 10, 1881, in which there were symptoms resembling those of myelitis. I desire to emphasize that peculiarity in cases of lead poisoning. We have known all along that the legs may be paralyzed as well as the hands, but it is not so generally recognized or mentioned that lead poisoning may almost exactly imitate myelitis. The value of an examination of the urine for lead after administering iodide of potassium becomes, then, self-evident. The lead line was wanting in most of the cases reported, and is often absent in undoubted cases of lead poisoning.

In only one of the four cases could the origin of the lead be discovered. This is perhaps not strange in view of the results obtained by A. Gautier (*Gaz. Hebdomadaire*, Nov. 11, 1881, p. 724), who examined different articles of food and found lead in preserved vegetables, fish, crustacea, meats, also in drinks and drinking water, and the water artificially charged with carbonic acid, in acid drinks and condiments put up in glass, in vessels and utensils of tin, in the glazing of the walls and furniture, the woolen and silk of our clothing, the leather of our shoes, in enamelled cloth. In the oil in which sardines are put up he found a relatively large proportion of lead.

SOME OF THE FACTS OF LISTERISM, WITH A COMMENTARY.*

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IN opening this discussion I shall first state what Listerians claim for the method, and give the results obtained, not from selected cases, but in *all* their operations, extending over prolonged periods of time. I shall then give my own experience of the method. I shall next explain the principle of Listerism, which seems to be misapprehended by many surgeons in this country, and in consequence, a fair trial of the method is rarely given, with the consequent result of an almost total disbelief in the positive statements of some of the most prominent of foreign surgeons who have become converts to aseptic surgery. The objections urged against the method will then be considered. Finally, I shall endeavor to show that this, as well as all other modern wound-dressings, is dependent for its success upon certain general scientific principles.

It will be of course impossible to more than lightly touch upon the salient points of my subject, but I shall endeavor to bring prominently forward all that is really essential for its comprehensive discussion.

Lister and his followers claim that by the recognition of

* Read before the Phila. Academy of Surgery, June 4, 1882, to open a discussion on Antiseptic Surgery.

certain general principles suppuration may be avoided, or at least reduced to a minimum ; that at the present time certain procedures have been found to ensure this success better than others, but that as the method has been much modified in the past, it may very probably be simplified in the future ; and very justly maintain that *the indications remaining the same*, the methods may vary without the essentials of Listerism being altered. They contend that aseptic surgery does not consist in the use of the spray, or any special dressing, but in complying as perfectly as possible with certain principles. It is not pretended that, under favoring circumstances, essentially similar results may not be obtained *exceptionally*, but it is asserted that, under the most unfavorable circumstances, where, previous to the introduction of Listerism, certain operations were almost invariably fatal, a dangerous mishap is as rare now as a recovery was before. The Lister school has been unfairly charged with making very absurd statements, but my last sentence really contains what they actually claim.

Let us now examine the results obtained in *all* the cases treated aseptically by various surgeons.

In his least healthy wards, Lister for four years suspended the annual washing. He overcrowded them, having several patients in *one* bed. The dressings were untouched from six to eight days, with but *one* case of pronounced pyæmia. In the same hospital he treated another series of operation-cases, etc., with the ordinary methods then in vogue. They amounted to several hundreds, and covered a sufficiently long period of time to eliminate any probable sources of error. As compared with his *aseptic* cases, he had nearly *double the mortality, and that from septic disease*.

Volkman, of Halle, when he commenced Listerism, was in such despair on account of the unsanitary condition of his wards as to be on the point of demanding their closure. As

an instance, in 1872-3, he tried to save—and they must, of course, have been comparatively simple ones—16 cases of compound fracture, and 12 rapidly died of pyæmia. Under Listerism, from 1873 to 1877, he tried conservative treatment in 75 complicated fractures in 73 patients, *without a death*. Let me give *all* his operations at his clinic, from March 1, 1874, to March 1, 1877:

AMPUTATIONS, SIMPLE.			AMPUTATIONS, COMPLICATED.		
	Cases.	Deaths.		Cases.	Deaths.
Shoulder,	4	1 in 4 hours.	Double,	9	2, of both thighs.
Arm,	14	—	Serious	6	6 in a few hours.
Forearm,	23	—	multiple } lesions,		
Wrist,	3	—	During septi-		
Hip,	2	1 in 4 hours.	cæmia,	15	8
Thigh,	42	1 in 24 hours.	One case at 60 years, died of delirium tremens.		
Leg,	25	1, erysipelas.	One tubercular case, died of pneumonia.		
Parts Foot,	42	—	One shoulder, aborted, and died of puerperal fever. In these latter cicatrization was nearly complete.		
	155	3, shock; 1, erysipelas.			
RESECTIONS, SIMPLE.			SECONDARY AMPUTATIONS.		
	Cases.	Deaths.		Cases.	Deaths.
Shoulder,	7	—	Thigh,	1	—
Elbow,	2	—	Knee,	4	—
Wrist,	2	—	Leg,	42	—
Hip,	48	4 { 1 child, 9 mos. 1 child, 2½ yrs. 1 after 3 mos.	Arm,	6	—
Knee,	21	1, tubercular meningitis.	Elbow,	5	—
Ankle,	5	—	Forearm,	15	—
	85	5 in all.		73	
Four excisions in pyæmic cases, <i>all fatal</i> .			Hydroceles incised,		
			45 cases, no deaths.		

	Cases.	Deaths.
Resections for false joints or vicious callus,	10 (9-1).	—
Osteotomies,	50 in 38 cases.	1, in a bleeder.
Amputations of Breast,	119 in 115 cases.	2, from erysipelas: one arising from a neglected dressing, one from a bed-sore.
		1, shock.
		2, exhaustion (pyæmia?).
		1, malignant pustule.

If dissatisfied with the statements of continental surgeons, examine the opinions expressed by such men as Spencer Wells, C. Macnamara, Barwell, T. Smith, J. Wood, Jonathan Hutchinson, Sir James Paget, Knowsley Thornton, etc., who, either in all operations or in certain of the more dangerous, extol Listerism in the highest terms.

Mr. Wood says: "I estimate highly the spray and gauze treatment in the opening of psoas, spinal, or other large abscesses for the first time."¹ J. Hutchinson says: "I have seen large abscesses opened under the spray and dressed with gauze, which did not suppurate further, and which behaved in a way wholly unexampled under the older methods of dressing. I have also seen many large operation-wounds so treated heal without suppuration, and without the slightest approach to a febrile condition." Paget says: "But there are some groups of cases in which I believe it would be absolutely wrong to dispense with any of the precautions of the complete antiseptic surgery. Among these are the cases of ovariectomy. * * * As with ovariectomy, and, I may add, all abdominal sections, so with osteotomy and the cutting into healthy joints. * * * These may now, with antiseptic precautions, be done with an almost complete safety."

"A few years ago I believed that I had never seen a patient recover after the opening of a lumbar or psoas abscess with a free incision. I could not remember one who had not died before the opened abscess healed. Of late years I have known such abscesses opened with complete impunity under antiseptic treatment, and there has seemed nothing but this treatment to account for the difference of the results."

I will now examine in detail the results of aseptic treatment in amputations, injuries of joints, compound fractures, etc.

¹ He means when not *previously* opened by the knife.

Scrutinize Schede's statistics of septic and aseptic cases. Thus, of 377 amputations performed by Bruns, Bardeleben, and Billroth, there was a mortality of 29.18%, while of 321 amputations performed by Busch, Schede, Socin, and Volkmann there was a mortality of 4.4%.

Bardenheuer had no death during a year at the Cologne Infirmary, with the following operations involving bones.

- 41 amputations (17 of thigh) through bones.
- 10 " at joints (1 at hip).
- 53 excisions of joints (15 hip, 12 knee).
- 23 cases removal wedge-shaped pieces of bone.
- 5 operations for badly united fracture.
- 1 case trephining.

As to joint cases, Bryant has criticised the small number of healthy articulations opened by Lister, and that several were of the smaller joints; but Mr. Lister has frequently opened those affected with hyarthrosis, impending suppuration, joint abscesses, etc., in all 109 cases, with but three deaths, all from tubercular meningitis. Volkmann gives 31 wounds of joints and 2 incisions into joints, 33 in all, of knee, wrist, elbow, and foot, with no death. He also gives 21 comp. fractures entering joints treated aseptically.

Resection was resorted to $\left\{ \begin{array}{l} 2 \text{ primarily.} \\ 5 \text{ secondarily.} \end{array} \right.$

Amputation was required—3 secondary amputations.

10 perfect recovery, with free motion.

1 ankylosis (not seen for 10 days).

—
21 No deaths.

In four cases late treatment or gangrene compelled operation.

Nussbaum says: "This method opens up a new field to surgeons. By the aid of these precautions, joints and cavities of the body may be opened without danger"; this, too,

in an unhealthy hospital. Hear Heuter: "It is with reason that I said, in 1870, that puncture of joints ought to be considered as a dangerous operation; it is with as much reason that I now affirm (1876) that one can practise this operation without danger."

After omitting all cases of compound fracture dying within 48 hours, or which required primary amputation, taking *all* Mr. Lister's hospital cases from 1871 to 1881, there are left 39 fractures in 38 cases with only 2 deaths, one of which was independent of the wound; 31, or 81.5 % were rendered aseptic, and *all* of them recovered, viz.: 31 compound fractures of the skull and long bones. In 82 compound fractures made by Mr. Lister, nearly every case pursued an aseptic course, but in one of these amputation was required; *none died*. These operations were performed upon the femur 27 times; leg, 31 times; clavicle, 3 times; humerus, 9 times; forearm, 11 times; and lower jaw, once—82 operations in 58 cases. The favorable results are not due to the *youth* of the patients, for they averaged as such cases do. Besides, he has had some 22 other operations on bones of such severity as to be dangerous, without a single fatality.

MacEwen reports an extraordinary series of osteotomies. His operation-wounds varied in length from $\frac{3}{4}$ inch to $1\frac{1}{2}$ inches, and were held apart so that the air had free access to the bones. He has operated on 330 patients, producing in them 885 compound fractures, but in only 8 did suppuration ensue. In only one instance was secondary amputation necessary, and here gangrene was produced by displaced bandages. Only 3 died out of 330 patients, one of whom had 10 compound fractures, the deaths being from diphtheria, tubercular meningitis, and pneumonia contracted before the operation. Most of his cases were also cachectic.

The accidental compound fractures of Lister, Volkmann, Schede, and MacCormac, in which an attempt was made to

purify the wound, excluding all dying within 48 hours, amount to 164 cases, of which number only 2 died. Of 158 fractures of the extremities, secondary amputation was required in 15, secondary excision in 5, leaving 138 cures with useful limbs. The intentional compound fractures of Lister, Volkmann, MacEwen, Bardenheuer, and MacCormac, number 530 cases with 1,072 fractures, with but 3 deaths, all of which were independent of the operation itself. In 766 limbs 1,068 fractures were made, and but 4 cases required amputation. Again, adding both classes together, of 1,226 fractures of 924 limbs, 902 recovered, but 19 required secondary amputation, and 5 secondary excision. Of these, 5, or 0.72 %, died. Contrast this with the best results of compound fractures treated by the ordinary methods, viz.: 22 %, which were achieved at St. Thomas' Hospital. Finally, examine the results following free opening of psoas abscesses. By considering those lost sight of in Mr. Lister's report as dead, we have a mortality of only 9 in 37, or 24.3 %. Some of the deaths were also due to phthisis, etc. This operation—which requires the greatest skill and attention to details—is that which has failed oftenest in other than Lister's hands, yet as Paget, Hutchinson, and Wood have pointed out, success *is* attainable. Can any candid mind refuse to believe statements coming from so many and varied sources? Many more such records of facts could be adduced in support of my position, but if what has been already said does not carry conviction with it nothing will. I imagine that no one present has ever succeeded in performing 72 major secondary amputations without a death. The surgeons who have given these statistics are implicitly believed when publishing other than *Listerian* facts. Upon what grounds, then, have we the right to deny them credence where aseptic surgery is concerned?

I will not tire you with details of my own experience. My failures were many at first, my success was slow, but I am now confident that I can do still better. The traumatic fever was insignificant, the pain slight; the wounds remained *white-edged*, unswollen, and healed largely by first intention; the pus was insignificant and different from what was ordinarily seen; the wounds were perfectly sweet, even in hot August weather, after 7 days; when sloughing of flaps occurred the discharge was as nothing, comparatively, and *with no odor*.

My statistics although quite extensive are, in my opinion, too small to be of any such determinate value as to warrant publication, although convincing to myself of the value of Listerism. I may say that my best results were obtained when the wards were overcrowded with suppurating wounds to a degree which should have certainly, according to Erichsen, generated pyæmia, etc.

What is Listerism? It is the prevention of putrefaction in wounds by the destruction of particles which induce fermentation, and which Lister considers are micro-organisms. Lister recognizes three main causes of suppuration, which, when overlooked, as they seem to have been by many surgeons in their application of his method, ensure failure, viz.: (1) Excessive tension of tissues; (2) direct irritation of the living tissues by the presence of a foreign body; and (3) direct irritation by germs which are usually suspended in the air. The omission of attention to the first two causes has usually resulted in disappointment, and in consequence the value of Listerism has been denied. A single tight stitch may, and failure to provide the most perfect outflow for the serous discharges, which are peculiarly large from the irritant effect of carbolic acid, certainly will, *ensure* suppuration, although it may be aseptic. Again, irritation of the wound by deluging it at the outset and at each

dressing with carbolic acid solution is almost *certain* to initiate the second cause of failure. The removal of the third cause of suppuration—and here is the point which most anti-Listerians overlook and then maintain that other methods besides the spray result in equally good results, *ergo* Listerism is false—may be effected either by the purification of the air, with consequent destruction of germs *before* they reach the wound; or, allowing them *free access* to the cut surfaces, they may be afterward *destroyed by germicidal applications*, and subsequently kept out by appropriate dressings. What I have just described is Listerism, or the principles of aseptic surgery. The *Listerian method* consists at present of spray, gauze, etc., but this is only one method of application of the Listerian principle, which, as it has varied in the past, may do so in the future. The mistake so commonly made is, that the spray, gauze, etc., constitute Listerism, *i. e.*, a certain *principle* of wound-treatment; forgetting entirely that Lister used none of these things in the past and yet got good results, that Trendelenberg uses no spray yet is a strict aseptic surgeon, etc., etc. When a man dispenses with the spray the cry goes up he has abandoned Listerism. He *has* abandoned the present Listerian *method*, but *not* the principle in most instances. Lister himself merely considers the spray as a *convenience*, which ensures results easily, and obviates the necessity for rapidity in operating and the deluging of wounds with an irritant like carbolic acid.¹ The most essential part of the theory is the thorough purification of every thing—instruments, hands, finger nails—which comes in contact with the wound, *and keeping them so throughout the operation*. The introduction of one infected instrument into the wound is vastly worse than the momentary deflection or cessation of

¹ Lister has pointed out that with good hygienic surroundings the air probably contains very few germs, certainly *none* compared to those lodged upon instruments, adhering to the hands, etc.

the spray. If Lister had to give up any part of his *method* (mind, not principle) he would abandon the spray, because it is both theoretically and practically possible to render a wound aseptic without its aid, although we can have no *certainty* of attaining this end. From this we can see how many wound-treatments are practically aseptic, and why results analogous to Lister's are obtained by surgeons who ridicule, while in reality profiting, wittingly or unwittingly, by, not his *method*, but his *principles*.

The chief objection urged against Listerism is that equally good results are attainable by means of cleanliness and hygiene. To this I would merely oppose the various facts that I have given, and especially that Nussbaum, with open method, disinfectants, etc., could get no such results until he instituted strict aseptic surgery, and that where this is impossible, he still has his *old accidents*. As von Buhl makes all the *post-mortem* examinations, and many practitioners and students see the cases, error can hardly arise.

Other objections from improper applications of Listerism I shall presently discuss. Another complaint is that carbolic acid is a poison. True, but almost never, and then chiefly when used antiseptically, not aseptically; *i. e.*, deluging the wound with quantities of the lotion. But Listerism does not consist in using carbolic acid. Salicylic acid, oil of eucalyptus, and acetate of alumina¹ are nearly as good. In passing, I would say, as a proof of the aseptic method, Volkmann tried thymol instead of carbolic acid, when he rapidly began to get bad results from the use of this inefficient antiseptic, although the same cleanliness, etc., was observed. Again, expense is urged, but this is hardly true, as fewer dressings, greater rapidity of cure, the rare need for tonics, febrifuges, etc., etc., and the shorter stay in hospital, render

¹ I have used this with quite as good success as carbolic acid.

it in the end no more expensive. Finally, it is said to be troublesome. After a little practice it is not more troublesome than other methods—and if it were, no right-minded man could seriously urge such an objection if the results are as they are stated to be.

There is one objection which is so illogical that I need merely mention it, viz. : that organisms are found under the dressings, *ergo* Listerism is a failure ; or, in other words, the reasons assigned for success are untrue, therefore the success, which is demonstrable, is a delusion.¹ Did the earth cease to go round the sun because the older philosophers taught the reverse ?

As time would fail for a thorough consideration of the subject, I will merely state a few bald facts as to the germ theory of putrefaction, referring my hearers to Cheyne, Pasteur, and others for details. One misapprehension must first be cleared up. Many surgeons confuse the germ theory of putrefaction with the germ theory of infective diseases, considering that they stand or fall together. This is a mistake, as there is no such necessary connection.

Fresh air frequently renewed, *i. e.*, oxygen, is more unfavorable to the growth of organisms than any dressing which imperfectly excludes the air. Indeed, Pasteur considers oxygen to be fatal to the development of the *vibrio septica*. In other words, the open method is better than any form of closed dressing, except Lister's, in preventing the growth of organisms. Concentration of wound-fluids interferes with their development, thus explaining, in part, both healing under a scab, Guérin's cotton-dressing, and the open method. All forms of germs are not equally harmful, as micrococci seem *not* to favor putrefaction, but bacteria do. The growth of some organisms is inimical to

¹Stimson's experiments as to the efficacy of the spray are so manifestly faulty as to demand no refutation. No one thinks of *sweeping a room so as to raise a cloud of dust* while operating !

others. Bacteria do not develop from micrococci; these act differently with chemical agents, and have been seen to divide *longitudinally*, while bacteria do not. Bacteria flourish in alkaline fluids. Micrococci flourish in a proportion of carbolic acid which almost totally prevents the development of bacteria.

Contact with healthy tissues destroys micro-organisms unless they are present in large numbers. In acute inflammatory affections or disease, organisms may gain access to the economy by other routes than by wounds. Organisms are always absent from chronic abscesses, but are equally constantly found in acute abscesses. If the latter are not opened early, these organisms are dead, since they soon exhaust their nutriment; they can be seen microscopically, but fail to develop when cultivated. Carbolic acid, unless much concentrated, does not kill, but *anæsthetizes* the germs and they cease to develop.

Finally, as to the presence of micro-organisms in wounds treated aseptically. When the method has been properly carried out they do not appear until late in the treatment of the case, when the *dressings have been left on for a long time, i. e.*, the carbolic acid is, to a great extent, washed away or has evaporated. They appear first at the edges of the dressings, and travel from *without inward*. Where the wound remains aseptic *nothing* but *micrococci* are observed. Now note three things, viz.: micrococci are found in acute abscesses, the pus of which is *sweet* when opened; when in small numbers they, as well as bacteria, are killed by contact with healthy *uninflamed tissues*; carbolic acid, like chloroform, will anæsthetize them, as it will the yeast plant, so that their destruction is not absolutely essential; Onimus and Dumontpallier showed long ago that minute proportions of various antiseptics destroyed the septicity, as proved by inoculation, of previously septic blood, with-

out apparently affecting the *vitality* of the micro-organisms. The bearings of these facts upon the explanations of the success of the open, modified aseptic, and aseptic methods of wound-treatment are too manifest to need comment.

Examine with me the pathology of wounds, and its bearing on the reasons for the success of certain plans of treatment, bearing in mind the just-mentioned facts regarding micro-organisms. For those that repudiate the germ theory of putrefaction, I need only say that the measures which have proved most efficacious in excluding organisms from wounds have also excluded the causes of putrefaction, and that, as I hope to show, the results can be satisfactorily explained upon another basis, although I am inclined to the germ theory myself. I shall now quote extensively from a paper published by me just a year ago.¹

"Let us first examine the condition of an amputation-wound after it has been put up. The capillary and smaller vessels are sealed by clots which extend to the next collateral branches; but, of course, these same vessels, where not filled with hæmostatic plugs, are in an excellent condition to absorb any thing brought into sufficiently close proximity. In a few hours, especially in women and children, where, owing to the thinness of the epidermis, the congestion is not concealed, a marked flush is seen. This increased vascularity is, of course, present in the deeper structures also. In the normal course of events this congestion is relieved by a more or less copious flow of serum, which is finally substituted by pus. As early as the third day—and I think that I have observed this change even before in quite a considerable number of cases,—by the aid of a lens, granulations may be seen springing up in those portions of the wound where perfect coaptation has been impossible.

"Let me recount one more set of preliminary facts experimentally proved by Billroth, viz., that an artificially prepared septic fluid can be placed in intimate contact with a *perfectly healthy granulating surface with impunity*; while another portion of the same, when injected into the cellular

¹ "An Analysis of the Principles on which Depends the Success of the Modern Methods of Treating Wounds," by C. B. Nancrede, M.D., ARCHIVES OF MEDICINE, June, 1881.

tissue, will rapidly destroy the animal, with the most marked septic symptoms.¹ He also states that since in compound fractures he has adopted the use of the plaster-dressing, the occurrence of septicæmia and pyæmia has been much less frequent in his practice.² By the more perfect quietude insured by the fixed dressing, the granulations are not mechanically injured by the frequent, although slight movements of the fragments; the muscles are maintained in a state of quietude, and consequently the pus cannot be either worked into the cellular interspaces, nor into the granulations, or breaches in them produced by movements of the fragments and injudicious handling. Now what is the most natural inference from this? Plainly, that which experience has already taught us, that perfectly healthy, well-formed granulations are an efficient barrier to the absorption of septic materials, at least for a time. I do not deny that acrid septic fluids, whether produced by the addition of a special ferment from without, or generated by bad hygienic surroundings, systemic conditions, etc., may, like caustic, rapidly destroy healthy granulations and thus allow of poisoning of the system, but I am prepared to maintain that for a considerable time they do prevent absorption. Besides, I have been so very often told by those who should know better, that a given wound was doing beautifully with a perfectly healthy granulating surface, with the qualifying statement, however, that it refused to cicatrize as it should, and upon inspection have found that portion only of the professional opinion concerning the 'not healing' was correct, that I am always sceptical when I hear that a thoroughly healthy surface in all its parts immediately preceded septic trouble. It should also be borne in mind that all structures do not form granulations with equal rapidity, so that while the accessible portions of a wound may be to the eye healthy, other deeper parts may be in an exactly opposite condition.³

"Now let us examine the conditions of the wound, during the first forty-eight hours, as to sepsis. Whether we believe that suppuration—especially unhealthy suppuration—and septic trouble are produced by the importation of germs from

¹ Recent experiments upon this subject have given *apparently* other results; but they are clearly explainable in accordance with my statement.

² Viewed in the light of the above-mentioned experiment, the explanation of such good results is readily discernible.

³ A careful analysis of the course of the majority of cases of blood-poisoning in all its forms, will demonstrate that the commencement of the trouble, *although unnoticed*, dates back to the first three or four days after operation or injury—indeed usually within the first forty-eight hours some *slight* symptoms appear, the gravity of which time demonstrates.

without or not, there can be no question that the secretions of a septic wound, when applied to solutions of continuity in other individuals, will produce a similar constitutional affection. What are the conditions most favorable to such absorption? Clearly the presence of a secreting surface capable of absorbing, and a favorable nidus for the development of the *materies morbi*, whether it be chemical or bacterial.

"The study of the physical conditions favoring osmosis shows us that an over-full condition of the vascular system, by the comparative stagnation of the contained blood, retards, if it does not entirely prevent absorption. This distended condition of the vessels I have already said soon supervenes after operation, and in accordance with this fact there is a variable but distinct period before any thing like marked fever shows itself. Soon, however, the overloaded vessels relieve themselves by a transudation of serum, or, perhaps more correctly, altered liquor sanguinis. This relative cessation of congestion acts in two unfavorable ways, however salutary it may in other respects prove, for it provides an abundance of a readily putrescible fluid well adapted for the propagation and development of bacteria and minute organisms, while at the same time it unloads the circulation to such an extent as, in the highest degree, to favor absorption. The normal vascularity of any part subjected to amputation is scarcely fitted for rapid absorption. The excessive primary vascularity just spoken of is still less favorable to osmosis, but the state of the blood-vessels, after their partial depletion, is such as to meet all the requirements for septic or other absorption. Soon, as we have seen, granulations commence to form, so that the risk of absorption is measurably diminished by the end of forty-eight to seventy-two hours. I would beg my readers to bear this fact in mind, as it gives the key, I believe, for explaining the success of von Brun's method.

"After these preliminary explanations, let us now—bearing clearly in mind the wound-conditions just described—examine what must be the indications to fulfil to avoid the risk of sepsis. Of course, no one doubts that if every wound could be made to unite by 'first intention,' or immediate union, the chances of septic infection would be almost, if not absolutely, done away with. To this end, the most accurate coaptation of the wound-surfaces, with their subsequent retention, is the primary essential. The most difficult portions to appose, and keep so placed, are the deeper parts, and the only agent likely to disturb the position of the flaps, after coaptation has been secured, is the accumu-

lation of the secretions, due to some obstacle presented to their escape. Manifestly, then, the second indication is to secure free drainage. The third indication is clearly to remove inflammatory products as soon as secreted, so as to render impossible any accumulation of that putrescible fluid poured out from the cut surfaces, which affords such an admirable nidus for the development of septic poison.¹ This removal is of paramount importance, as is clear from our previous studies, for the first forty-eight or seventy-two hours. This indication is plainly fulfilled by free drainage. Fourthly, if possible, the secretions of wounds—which must be present to some extent—should be kept from chemical change, and rendered unfit for the generation of septic matters, by agents which, if possible, shall promote the formation of granulations. To sum up, the indications are: perfect coaptation, perfect drainage, disinfection, promotion of granulation, and rigid avoidance of any thing that can chemically or mechanically injure these safeguards of nature.

“Now, let us see how each one of the three typical methods of wound-dressing fulfils these indications, and whether the measure of success attained by each is not to be attributed to the degree of strictness with which they are carried out.

“Lister” inculcates the most perfect coaptation, drainage, and disinfection which is practicable, with the least possible manipulation and after-dressing. Undoubtedly the carbolic acid, by coagulating the albumen in the tissues, forms a superficial, antiseptic, comparatively impermeable film, which, like that formed by chloride of zinc, remains until, by the formation of granulation, the slough—for such in truth it is—becomes separated. The antiseptic method certainly renders the wound-fluids unfit for the propagation of the septic poison; whether by preventing the formation of minute organisms or not, is beside our present purpose to consider.

“On the other hand, the ‘open method’ succeeds by very dissimilar methods in fulfilling some of the indications perfectly, while others it professedly and necessarily does not attempt. I say necessarily, because their fulfilment would inevitably prevent the carrying out of the others.

“Let us examine this method, as we have just done Listerism. The first indication, viz., as perfect coaptation as possible, is of necessity neglected, so that whatever advantage accrues from healing by primary union, is lost. In return for this, its advocates claim—and rightly—that the

¹ Experiments have proved that pressure favors absorption, so that this accumulation is dangerous on this account also.

coaptation secured, although imperfect, is not disturbed by the accumulation of the discharges, and that they are prevented from remaining in contact with an absorbing surface for any considerable length of time, or in any quantity. By this removal, as soon as effused, of the wound-fluids, they maintain that chemical and septic changes are very unlikely, and should they obtain, do no harm, because the poison cannot remain in contact with an absorbing surface sufficiently long.

"I admit that the second and third indications, which amount to free drainage to prevent separation of wound-surfaces, and free drainage to obviate the risk of absorption by the long retention of the wound-fluids in contact with absorbing surfaces, are admirably fulfilled, but the fourth indication is certainly not fulfilled, other than negatively, if the strictly open method is used. The disadvantages inherent to this plan will be presently adverted to. The advantages of this method are clear and unmistakable, but are not dependent upon any thing but the fulfilment of the principles pointed out, and upon which also depends the success of the antiseptic method.

"Finally, there remains for consideration the plan lately advocated by von Brun. Dispensing with the spray and the other elaborate formalities of the Lister operation, after its completion the stump is syringed out with large amounts, say half-a-gallon, of weak carbolized water: it is then dressed antiseptically, and left undisturbed for forty-eight hours. The dressings are then removed, and the stump treated by the open method. Subjected to the same critical analysis as the other two systems, we find that the most perfect coaptation possible, drainage by tubes, etc., prevention of chemical or other changes in the fluids, and treatment of the wound-surfaces by a coagulating agent, are relied upon, *and that this antiseptic condition is kept up for the first forty-eight hours*; in other words, until most of the primary danger is passed: because, as we have seen, healthy granulations are a protection against septic absorption, and their formation has commenced, and, in many cases, is well advanced by the time specified. At the end of the first forty-eight hours the troublesome details of the Lister antiseptic dressing are dispensed with, and the open method substituted. It is hardly necessary to point out that most of the good to be obtained by a Lister dressing is here combined with the advantages of the open method. In this plan, too, there is nothing mysterious, nothing occult, but the simple successful employment of certain well-established surgical principles.

“Look at the results of Alanson’s famous series of major amputations performed nearly a century ago. He coaptated the deep surfaces of the flaps by compresses, leaving the edges comparatively to themselves, thus securing the inestimable advantages of quick healing, free drainage, and—because nothing could accumulate in amount—freedom from septic absorption. Examine the records of Callender’s admirable series of amputations, and upon what did his success depend? Was it not upon free drainage, absolute quiet of the limb, treatment of the wound surfaces with a solution of chloride of zinc, thus forming an impenetrable antiseptic slough, which, upon separating some days after the operation, left a healthy granulating surface, and, finally, upon rigid avoidance of any thing that could injure the granulations, merely cleansing them with a camel’s-hair pencil dipped—and kept in the intervals—in an antiseptic solution? The limb, where opportunities served, was swung for some days before the operation, which, of course, rendered the patient accustomed to the new position. This, in part, obviated those movements which Billroth has shown are apt mechanically to work the pus into the intermuscular spaces, where it can be absorbed, since it must then have broken down the barrier of the granulations.

“I maintain that the success of all these definite methods of wound-dressings,—indeed, the good result achieved by every surgeon, is necessarily dependent upon the degree in which he succeeds, wittingly or unwittingly, in fulfilling the indications pointed out in this paper.

“Provided Lister’s directions are properly carried out, his method, though at first troublesome, seems to me both theoretically and practically the best. To operate satisfactorily it demands skilled assistance, some experience, and the utmost attention to detail, combined with a thorough appreciation of the principles involved. By excessive care upon the part of the operator, skilled assistance may be dispensed with, although it renders the labor greater, and risks the success of the operation. It is utterly unfair to deliberately transgress those rules upon which Mr. Lister lays most stress as being essential to success, and then condemn his method. He distinctly says that carbolic acid is an irritant to the tissues, and, therefore, by a special protective specially tested, he keeps it from direct contact with the wound; otherwise suppuration will occur. As the spray itself is an irritant—any operator’s hands will demonstrate this,—special provision for draining off the excessive amount of serum poured out must be provided; yet, surgeons will almost hermetically seal a wound by stitches,

straps, etc., thereby rendering suppuration from tension certain, when Mr. Lister has said that a single stitch producing tension will provoke suppuration. In this latter case, however, the pus does not become offensive.¹ Again, the less of the direct spray coming in contact with the raw surfaces the better, and yet we see a powerful atomizer within a couple of feet of an open abdominal cavity, the spray playing directly upon the exposed peritoneum, cooling it, if not doing other fatal damage, when the cry is again raised that Listerism is worse than useless. But what I have just described is *not* Listerism. The magnificent triumphs in recent abdominal surgery, considered by the most noted operators to be due to the use of this method, plainly demonstrate what the method in proper hands is capable of. An ovariectomy can be usually done with impunity, with strict Listerism, in a general hospital, where, without it, certain death has been proven to be the almost inevitable result."

In considering such an important subject as wound-treatment, no personal bias should be allowed to influence us. As Paget has remarked, an immense improvement in the success following operations had taken place before the advent of Listerism, and he thinks that experience has demonstrated that from 60-70 per cent. of patients are insusceptible to infective disease. That hospitalism is the only cause of pyæmia or septicæmia, and that mere cleanliness is the all-essential preventive, have been disproved by the statement of Prescott Hewitt and many other surgeons as to their experience in private practice, and I have myself witnessed it on more than one occasion. Admit all that the opponents of Listerism advance concerning cleanliness and hygiene, if Listerism will practically abolish infective diseases amid the worst surroundings, so that the results of operations at least equal those performed under the

¹ During the hottest summer weather I have dressed a bad compound comminuted fracture of the leg, where profuse suppuration had resulted from inadequate drainage, at intervals of from *four* to *eight* days, without the faintest trace of putrefactive odor being perceptible. Other similar cases could be cited which have been treated by the author, but this will suffice to prove the truth of my statements.

best circumstances, will not the application of the method with the most favorable surroundings prevent *all* that is preventable, viz.: all except those rare instances where blood poisoning has not its origin in the operation-wound?

Statistics can go but a short way to prove the advantages of any mode of treatment. The rapidity of recovery, condition of the wound, which is supple and non-indurated, the slight fever, the absence of necessity for change of diet, lessened drain, slight pain, etc., must be taken into consideration as favorable to asepticism, and if such results are considered desirable the method will sooner or later be adopted by the majority of surgeons.¹

¹ I have not attempted to refer to the various authors that I have mentioned specifically, but I here acknowledge my indebtedness to MacCormac, Van Buren, Cheyne, and others. The reader is referred for many additional facts and arguments elicited during the discussion following the reading of this paper. They will be found, in brief, in the *Phila. Medical Times* for July 29, 1882.

NOTES ON TWELVE CASES OF BRAIN TUMOR, CHIEFLY WITH REFERENCE TO DIAGNOSIS.*

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THE notes presented are on twelve cases of intracranial tumor, in which the clinical and *post-mortem* examinations were made by myself. Five of the cases have already been published in detail in various journals, to which references are made below; seven have not before seen the light of publication. I have endeavored to condense the clinical histories and the *post-mortem* and microscopical notes into the narrowest limits compatible with an intelligent understanding of the cases.

CASE I.—*Clinical History*: Male, æt. 35. History of syphilis. Fell from door-step, striking head. Headache persistent and violent. Vertigo. Mental slowness and uncertainty; attention destroyed; hysterical sobbing and crying. Nystagmus; tonic spasm in muscles of neck and forearm. No true paralysis, but movements weak and uncertain. Speech explosive. Blindness which came on gradually, first in right eye then in left. Conjunctivitis in left eye; corneitis with tendency to sloughing; sensibility of conjunctiva and cornea lowered. Choked disks. Smell defective. Polyphagia. Constipation, alternating with involuntary evacuations. Urine passed involuntarily. Temperature from 97° to 101° F.; higher in the evening. Head-temperatures above normal; highest at frontal station, averaging 96.5° F.

* Read before the American Neurological Association, June, 1882.

Pathological Anatomy: Fibroma. In the right frontal lobe, involving the anterior upper half of the first frontal convolution, the anterior upper and inner half of the second frontal, a small segment of the gyrus fornicatus just where it bends round the knee of the corpus callosum, and the anterior half-inch of the corpus callosum itself. The membranes over the tumor were destroyed. Microscopical sections from the posterior part of the eyes at the embrace of the optic nerve presented the appearance usually encountered in choked disks.

Remarks: *Philadelphia Medical Times*, January 18, 1879, in Proceedings of the Pathological Society of Philadelphia.

CASE 2.—*Clinical History:* Male, æt. 36. History of syphilis. Headache, continuous and nearly always excruciating. Head sensitive to pressure everywhere, but most in frontal and left parietal regions. Faculty of attention wanting; continuous mental action impossible; often crying. General hyperæsthesia. Sight impaired; pupils contracted. No ophthalmoscopic examination. Constipation. Fever with delirium; finally sopor and coma. During last week of life the pulse ranged from 90 to 110; temperature from 100° to 101.5° F.; respiration from 20 to 34.

Pathological Anatomy: Three gummata. (1) The largest growth, 1¼ in. in diameter, adherent to pia mater, in the right præfrontal region, involving the superior anterior portion of first and second frontal convolutions. Pia mater adherent to dura over tumor; a cheesy mass between dura and bone; strong adhesions between dura mater and skull; some surrounding pachymeningitis. (2) A growth, ⅔ths of an inch in diameter, adherent to pia mater, which was fused with the dura, in the retrocentral fissure, ½ inch behind the junction of the upper and middle third of the fissure of Rolando. (3) A growth ⅝ths of an inch in diameter, adherent to pia mater, in the supramarginal convolution, at the extreme posterior point of the horizontal branch of the Sylvian fissure. Dura mater adherent to the calvarium at several points over both hemispheres.

Remarks: Not before published.

CASE 3.—*Clinical History:* Female, æt. 35, white. History of syphilis. Headache, severe, at first with long intermissions; later continuous, and often agonizing. Vomiting with paroxysmal headache, coming on late in illness. Vertigo, usually with headache. Excitable and irritable. Slight twitching of mouth, hands, and forearms. Left hemiplegia; upper as well as lower fibres of facial nerve paralyzed. Left internal strabismus. Electro-contractility

diminished. Difficulty in enunciation. Sensibility diminished in left leg. Late hyperæsthesia, and great pain in paralyzed limbs. Conjunctivitis and necrosis of cornea of left eye; conjunctiva and cornea insensitive. Sight impaired. Descending optic neuritis. Hearing impaired on left side. Smell defective. Profuse perspiration, more marked on right side than on left. Constipation. Temperature, taken in right and left axilla for eleven weeks before death, gave the following averages: Right axilla, 99.1° , M.; 100° , E. Left axilla, 99.4° , M.; 101.4° , E. On some days remarkable falls in temperature to 96° , 95° , and even 94° and 93° . Average head-temperatures above the normal: for stations on right side of head averaging about 97° ; on left side, about 94.3° F.

Pathological Anatomy: Gumma. A twin-tumor, each mass about $\frac{2}{3}$ ds of an inch in diameter, in front of the optic chiasm. The growths involved the basal termination of the corpus callosum, the peduncles of the corpus callosum, the lamina cinerea, and anterior perforated spaces. They also probably encroached upon the roots of the olfactory nerves, the optic nerves and commissure, and the anterior portions of the circle of Willis, which seemed to have been broken in front. The base of the brain, from the posterior line of the tumor backward to the pons, was markedly softened. Microscopical sections of optic nerves showed the appearances peculiar to descending neuritis in a somewhat advanced stage.

Remarks: *Philadelphia Medical Times*, March 23, 1879, in Proceedings of the Pathological Society of Philadelphia. Also *New York Medical Record*, August 9, 1879.

CASE 4.—*Clinical History:* Female, æt. 30. No history of causation. Headache, continuous, sometimes agonizing. Percussion of head caused most pain in right parietal region. Vomiting when headache was most severe. Vertigo. Mind clear, but acted slowly; emotional. Spasm, beginning with twitching of fingers of left hand; most severe on left side, and especially in left arm. Upper as well as lower fibres of left facial nerve partially paralyzed; nearly complete paralysis of left arm; slight paralysis of left leg. Bowels and bladder partially paralyzed. Impaired sensibility in limbs of left side. Left patellar reflex diminished. Sight very imperfect. Choked disks. Hearing defective in right ear.

Pathological Anatomy: Carcinoma. About $1\frac{1}{2}$ inches in diameter beneath and adherent to the pia mater of the convexity of the right hemisphere; the pia and dura mater were united by strong adhe-

sions. The tumor involved the middle portion of the ascending parietal convolution and the upper part of the inferior parietal lobule, pushing aside the interparietal fissure. The anterior extremity of the tumor was about $\frac{1}{4}$ th inch back of the centre of the fissure of Rolando. On the inner side of the tumor the white matter of the brain was broken down.

Remarks : Reported at the meeting of the American Neurological Association, June, 1881, and published in ARCHIVES OF MEDICINE, August, 1881.

CASE 5.—*Clinical History :* Female, æt. 38. History of syphilis. Blows on the head. Headache, with agonizing paroxysms. Top and right side of head sensitive to percussion; and headache severest in these regions. Vomiting. Vertigo. Great mental irritability. Severe left-sided spasm, beginning with twitchings in left toes and foot. Partial paralysis of right leg and arm, most marked in leg. Hyperæsthesia. Impaired sight. Choked disks. Head temperature taken once : right parietal region, 97.2° F.; left parietal region, 96° F.

Pathological Anatomy : Gumma. Attached to the fused membranes of the right convexity. Involved the upper fourth of the ascending frontal, and a smaller segment of the ascending parietal convolution, crossing the fissure of Rolando at its upper extremity. A good example of strictly cortical lesion, involving only gray matter, and having only a very thin layer of softened tissue on its inner side. Microscopical examination of the optic nerves showed the appearances of choked disks, with probable ascending neuritis.

Remarks : Not before published.

CASE 6.—*Clinical History :* Male, æt. 31. Evidences of tuberculosis. Headache, continuous, with severe exacerbations; most severe at vertex. Vertigo. Some irritability and emotionality; hallucination that some one was going to come and take him away. Spasm confined to left arm. Partial paralysis of left arm and leg, and late in his illness, of left side of face. Left hemianæsthesia, at first partial, but later complete and persistent. Sight impaired; right pupil dilated and left contracted before death. No ophthalmoscopic examination. Hearing defective in left ear; tinnitus aurium. Head-temperature taken once : right frontal region 98° F.; left frontal region, 96.3° F. Cheyne-Stokes breathing on day of death.

Pathological Anatomy : Tubercular tumor. Dura and pia mater adherent over the tumor, which involved the posterior extremities of first and second frontal, and upper thirds of

both ascending convolutions, of right hemisphere. Interior of hemisphere broken down into a cavity containing brain detritus and some pus; the parts destroyed included white matter of the parietal lobe, the posterior third of the lenticular nucleus, and the adjacent posterior portion of the internal capsule. Miliary tubercles in pia mater around and near the tumor.

Remarks: Not before published.

CASE 7.—*Clinical History:* Male, æt. 25. Kicked on the head. Headache, nearly continuous, with violent paroxysms. Vomiting at intervals, most when headache was most violent. Vertigo. Mental confusion; sometimes maniacal. Left convergent strabismus. Partial right hemianæsthesia. Patellar reflexes slightly exaggerated; slight ankle clonus on the right side. Blind in both eyes; sight of right eye lost first. Choked disks, and eventually optic atrophy. Hearing defective in right ear. Constipation. Head-temperatures taken once: right parietal region, 98° F.; left parietal, 97.8° F.

Pathological Anatomy: Fibroma. A firm growth, 2½ inches in diameter, adherent to membranes, and involving the left occipital and postero-parietal region to within a half inch of longitudinal fissure. The brain tissue around and beneath the growth was softened and broken down, the parts disintegrated being chiefly the white matter of the postero-parietal and occipital lobes.

Remarks: Not before published.

CASE 8.—*Clinical History:* Male, æt. 33. History of syphilis. Blows on the head. Headache, continuous, usually dull, occasionally severe. Vomiting occasional. Stupidity, want of energy, drowsiness. One general convulsion a few hours before death. Partial anæsthesia on the right side. Hyperæsthesia of left side. Sight impaired. No ophthalmoscopic examination. Hearing defective in right ear. Constipation.

Pathological Anatomy: Gumma. Dura and pia mater adherent. Tumor 1¼ inches in diameter, attached to adherent membranes, and involved the middle portions of the first and second temporal convolutions of left hemisphere. A layer of brain substance both anterior and posterior to the tumor was softened. The tumor grazed, but did not involve, the ascending parietal and inferior parietal convolutions. A large *foyer* of greenish-yellow pus was found to the inner side of the tumor, the abscess having destroyed a considerable portion of the white matter of the temporal lobe.

Remarks: Not before published.

CASE 9.—*Clinical History:* Female, æt. 13. Tubercular history. Headache, with severe paroxysms; most marked in frontal and occipital regions. Vomiting. Vertigo; attacks of reeling and falling. Mental dulness. Weakness of limbs, but no distinct paralysis. Hyperæsthesia; severe pains in limbs. Gradually went blind, first in left eye, and then in right. Descending neuritis determined nearly a year before death; marked optic atrophy observed a month before death. Constipation.

Pathological Anatomy: Tubercular tumor. A large nodulated mass which occupied the lower two thirds of the right cerebellar hemispheres; one small nodule extended across the posterior extremity of the superior vermiform process, destroying altogether about one third to one half of a cubic inch of its substance. Internal hydrocephalus, the ventricles and the horns of the lateral ventricles being greatly dilated and filled with fluid.

Remarks: Not before published.

CASE 10.—*Clinical History:* Female, æt. 27. History of syphilis. Headache, with paroxysmal exacerbations. Vomiting, frequently recurring; for four weeks before death vomited almost incessantly. Vertigo; impossible to sit or stand because of vertigo late in illness. Slowness of mind, and hebetude. General muscular weakness. Right partial hemiplegia and hemianæsthesia. Trigeminal neuralgia. No ophthalmoscopic examination; but sight impaired. Marked slowness of respiration; respirations ran down as low as four and five per minute two weeks before death.

Pathological Anatomy: Nature of growth not determined. A nodulated mass, $\frac{3}{4}$ ths of an inch in greatest diameter, involving the right middle cerebellar peduncle and the adjacent region of the right cerebellar hemisphere. The right upper half of the floor of the fourth ventricle was superficially softened. An area of softening was also found involving a small portion of the under outer surface of the left optic thalamus, the entire breadth of the internal capsule, and a small segment of the lenticular nucleus.

Remarks: Not before published.

CASE 11.—*Clinical History:* Male, æt. 32. Thrown from a horse and kicked on the head. History of syphilis; headache, severe at first, always came on at night; became less severe later. Vertigo. Defective memory; apathy. Right hemiparesis; helplessness of all the limbs before death; partial ptosis of the right side. Sensation diminished in the left side of the face and in the right limbs. Pupils small before death. Descending optic neuritis. Conjugate

deviations of the eyes and rotation of the head to the right. Persistent epistaxis ; tendency to hemorrhage from mucous membranes. Constipation.

Pathological Anatomy : Gumma and fibroma. (1.) Gumma, half an inch in diameter, distinctly limited to the left upper quarter of the pons. (2.) Fibroma, no larger than a pea, between the dura and pia mater, causing a slight depression in the first temporal convolution about the junction of its middle and posterior thirds, and half way between the parallel fissure and the horizontal branch of the Sylvian fissure. Microscopical examination of the optic nerves showed a descending neuritis of subacute character.

Remarks : Reported at the meeting of the American Neurological Association, June, 1881, and published in the *Journal of Nervous and Mental Disease*, July, 1881.

CASE 12.—*Clinical History* : Male, æt. 35. History of syphilis. Wounded in head by glancing bullet. Headache of great severity at intervals. Vomiting at time of headache. Vertigo. Failure of memory and depressed spirits. Epileptiform attacks ; in the spasms lifted up the right side of his body and worked over toward the left. Occasional cramps in stomach and legs. Temporal and orbital neuralgias. Descending neuritis, and eventual atrophy of both optic nerves. Constipation. Died at the close of a series of convulsions, death being preceded by general paralysis.

Pathological Anatomy : Gumma. A rounded mass, quarter of an inch in diameter, attached to the pia mater, just to the left of the centre of the anterior surface of the pons. The entire anterior central portion of the pons was softer and more doughy than usual. At the junction of the pons with the medulla oblongata was a recent hemorrhage, which had spread downward to about the middle of the latter.

Remarks : *Brain*, January, 1880.

I will recapitulate and summarize, with a few remarks, some of the most important facts brought out in the foregoing reports :

Etiology.

History of syphilis in three cases.

“ “ “ and traumatism in five cases.

“ “ traumatism alone in one case.

Tubercular history in two cases.

No history in one case.

Pathological Anatomy.

Gummata in six cases.

Fibromata in two cases.

Tubercle in two cases.

Carcinoma in one case.

Not determined in one case.

The General Diagnosis of Tumors of the Brain.—I believe that the general diagnosis of the existence of intracranial tumors can be made with greater certainty than that of almost any other serious encephalic disease. It is true that cases have been reported in which autopsies have revealed the presence of large tumors, which are said to have caused no symptoms during life ; but I cannot get rid of the impression that such cases have been more or less imperfectly observed and recorded. The symptoms which point conclusively to the existence of an intra-cranial tumor are as follows :

1. Headache.
2. Pain on percussion of the head.
3. Vomiting.
4. Vertigo.
5. Mental disturbances.
6. Hyperæsthesia.
7. Choked disks or optic neuritis.
8. Elevated temperature of the head.
9. Constipation.

Headache.—Headache was present in every case. In ten cases more or less headache was present all the time ; and in all of these paroxysms of agonizing severity occurred. Absolute intermissions in the headache were observed in only two cases, and in one of these the pain became continuous before the end. The usual type of headache from intracranial tumor might be described as a continuous pain, sometimes of persistent severity, and generally with exacerbations of great violence. In two instances the patient

complained spontaneously of the pain being greatest in the region of the head nearest to the seat of the growth. In one of the cases of cerebellar tumor the patient referred the pain sometimes to the occipital and sometimes to the frontal region. Mr. Callender has noticed that cortical lesions, like those observed in these cases, are more frequently accompanied by localized pain than lesions of deeper parts (*Bartholomew's Hospital Reports*, 1869, and Ferrier's "Localization of Cerebral Disease," p. 99).

Percussion of the Head.—In three cases percussion of the head elicited an intensified pain in the region corresponding to the location of the tumors. As this procedure was only resorted to in four cases, the results favor the view that percussion of the skull may afford valuable aid in localizing encephalic tumors, particularly when they are meningeal or cortical. Ferrier (*Brain*, January, 1879) believes that percussion often elicits pain over the cranial region beneath which a lesion is localized. Dr. Alex. Robertson, of Glasgow (*Abstr. Internat. Med. Congress*, 1881, p. 85, and *Journal of Nervous and Mental Disease*, April, 1882), has also directed attention to the value of percussion of the skull in the localization of disease on the surface of the brain.

Vomiting.—Vomiting was a symptom in eight of the twelve cases. In Cases 1 and 2, in which the chief lesions were in the antero-frontal regions, vomiting was not present, although the tumors were of considerable size, and the other manifestations were of the most decided character. Even in Case 3, in which the tumor was well forward but low down, vomiting did not come on until late. In Case 6, a tubercular growth of the postero-frontal region, vomiting was absent; although in Cases 4 and 5, in which the lesions were in the motor zone near by, it was a prominent symptom. In both cases of cerebellar tumor, vomiting was prominent; one of these (Case 10) was the most remarkable

example of persistent cerebral vomiting which it has ever been my lot to observe. For four weeks before her death she vomited almost continuously.

I incline to agree with Ferrier (*Brain*, July, 1879) that the majority of cases of cerebral vomiting can be ascribed to irradiation of irritation of the nerves of the cerebral membranes, or to the physical effects of acute pain. The nuclei of the trigeminus and of the vagus are in close anatomical relationship in the medulla oblongata. Irritative impressions conveyed from the cerebral membranes, and particularly from the dura mater, to the trigeminal nuclei in the medulla, are transferred to the vagus and give rise to vomiting. In tumors of the cerebellum the vomiting may be brought about through the affection of the centres of equilibration, and the concurrent vertiginous sensations (Ferrier); or it may be due to direct irritation by contiguity of the floor of the fourth ventricle. In general, the tumors situated farthest forward in the brain appeared to be the least likely to give rise to vomiting.

Vertigo.—Vertigo was noted in ten cases. Under this general designation dizziness, giddiness, reeling, insecurity in the erect posture, etc., have been included. The vertigo which so constantly accompanies brain tumors can be explained in several ways besides the view of the affection of centres of equilibration in the cerebellum. Like headache and vomiting it seems sometimes to flow from pure irritation of the cerebral membranes. Alterations in the intracranial circulation produced by the growth of the tumor, is another probable cause. The experiments of E. Weber have demonstrated the existence of a communication between the arachnoid cavity and the labyrinth, and, consequently, the increased intra-cranial tension present may result in pressure on the labyrinthine fibres of the auditory nerve, and thus occasion vertigo.

Mental Disturbances.—Mental disturbance of some sort occurred in almost every case. When I say this, however, I am using the term mental disturbance in a very general sense. The majority of the patients were emotional, excitable, and irritable. It was not uncommon for them to sob and cry apparently from the great pain. They were usually easily affected by external impressions. One case (6) showed marked hallucinations of fear. Another case (7) had at times maniacal attacks, which usually coincided with the most violent paroxysms of headache. Some of the cases exhibited dulness, stupidity, want of energy, or apathy. Failure of memory, depression of spirits, and hebetude were other mental manifestations. Mental slowness and uncertainty, inability to fix the attention, and impossibility of continuous mental action were strikingly shown, particularly in the cases of tumor in the præfrontal regions.

Hyperæsthesia.—Hyperæsthesia was noted in five cases. It is a common symptom in brain tumor; sometimes it is general, more frequently it is present in the limb or limbs affected with spasm or paralysis. With the hyperæsthesia, the patients not infrequently complain of severe spontaneous pains in the limbs.

Choked Disks and Optic Neuritis.—Ophthalmoscopic examinations were made in eight cases. In four of the eight, microscopical examinations of the eyes and the optic nerves were also made. I wish to acknowledge special obligations to Dr. E. O. Shakespeare, pathologist and ophthalmologist to the Philadelphia Hospital, by whom the ophthalmoscopic and microscopic examinations were conducted in these four cases. The results in the eight cases were as follows:

Choked disks in four cases.

Descending neuritis in four cases.

Both eyes were affected in each case. In every case, therefore, in which the eyes were examined with the oph-

thalmoscope, marked changes were found. It will be seen that, with Allbutt and others, I make a distinction between choked disks, or ischæmia of the disks, and true descending neuritis. A brain tumor may give rise to either of these conditions, or to both in the same patient.

A careful examination of the reports will show that true choked disks were present in tumors of the convexity. In three of the cases of descending neuritis the tumors were situated at the base, and the fourth was a tumor of the cerebellum with hydrocephalus. Ischæmia of the disks may also have been present at some stages of these cases. Descending neuritis would seem to be most likely to occur when the tumors are so situated that the inflammation set up by them can readily extend by anatomical continuity to the optic nerves. The "lymph-space theory" seems to me to best account for the occurrence of choked disks in cases of tumor situated remotely from the base. "According to this theory, owing to increased intracranial pressure, partial stasis occurs in the veins, with exudation of the watery parts of the blood into the subdural and subarachnoid cavities, and this fluid, seeking the point of least resistance, finds its way into the lymph space between the outer and inner sheaths of the optic nerves, and, by gradual pressure, causes a dilatation of the peripheral end of the dural sheath just before it passes into the sclerotic, compressing, at the same time, the pial sheath and the nerve fibres covered by it."¹ The presence of choked disks or optic neuritis should be regarded as the strongest possible corroborative evidence of the existence of an encephalic tumor.

Temperature of the Head.—Observations on the temperature of the head were made in five cases (Cases 1, 3, 5, 6, and 7). Full accounts of the observations in two of

¹Optic Neuritis as a Symptom of Intracranial Disease, by William F. Norris, M.D., *Philadelphia Medical Times*, August 30, 1879.

these cases (1 and 3) have already been published (*Philadelphia Medical Times*, January 18, 1879, and *New York Medical Record*, August 9, 1879). It is not within the scope of the present paper to review the general subject of cranial or cerebral thermometry. I will simply, in passing, recall to the Association the labors of E. Seguin,¹ Broca,² Gray,³ Lombard,⁴ Maragliano and Seppilli,⁵ and Amidon.⁶

Dr. L. C. Gray (*N. Y. Medical Journal*, August, 1878) has recorded some observations in cerebral thermometry in one case of tumor of the brain. The tumor, a soft, jelly-like mass the size of a hazel-nut, was found between the horizontal branch of the Sylvian fissure and the first temporal fissure, while the whole of the right occipital lobe was converted into a colloid, extremely vascular mass, which gave way under examination, this degeneration also extending anteriorly to the tumor as far as the fissure of Sylvius. Microscopical examination showed the tumor to be a typical glioma.

The temperatures at different stations were as follows :

	Left.	Right.
Frontal	96.75°	98.33°
Parietal	95°	99.75°
Occipital	96.75°	100.50°

The average of the left side was 96.16°; of the right, 99.52°; of the whole head, 97.84°.

In my case of frontal tumor, reported in the *Medical Times* (Case 1), the average temperatures of the different stations taken were as follows :

¹ "Medical Thermometry and Human Temperature," by E. Seguin, M.D., William Wood & Co., New York, 1876.

² *Progrès Médical*, 1877.

³ *New York Medical Journal*, August, 1878.

⁴ "Experimental Researches on the Temperature of the Head," London, H. K. Lewis, 1881.

⁵ Quoted in *ARCHIVES OF MEDICINE*, 1879.

⁶ *ARCHIVES OF MEDICINE*, April, 1880.

Middle frontal station	96.5°
“ occipital “	95.5°
Right frontal “	95°
Left “ “	94.7°
Right parietal “	94.7°
Left “ “	94.4°

In the case reported in the *New York Medical Record*, August 9, 1879 (Case 3), a gumma which involved the basal termination of the corpus callosum, the peduncles of the corpus callosum, the lamina cineria, and anterior perforated spaces, the average temperatures at the different stations were as follows :

Right frontal station	97.1°
Left “ “	97.4°
Right parietal “	97.6°
Left “ “	94°
Right occipital “	97°
Left “ “	94.5°
Right anterior vertical station	97°
Left “ “ “	94°
Right posterior “ “	96.6°
Left “ “ “	94.3°

The normal average temperatures, as determined by Gray, were as follows :

Left frontal station	94.36°
Right “ “	93.71°
Left parietal “	94.44°
Right “ “	93.59°
Left occipital “	92.66°
Right “ “	91.94°
Left side of the head	93.83°
Right “ “	92.92°
The whole head, exclusive of the vertex	93.51°
Motor region of the vertex	91.67°
The whole head, inclusive of the vertex	92.66°

In Case 5, a gumma in the cortical motor zone of the right side, the head-temperatures, taken once, were found to

be for the right parietal region 97.2° ; for the left, 96° . In Case 6, also a tumor of the right motor zone, the right frontal region gave a temperature of 98° ; the left, of 96° . In Case 7, a tumor of the left occipito-parietal region, the temperatures were for the right parietal region, 98° ; for the left, 97.8° . Although the temperature of the head was only taken once in each of these three cases the observations were carefully made, with tested thermometers.

I conclude, from a study of the results obtained in these cases, that in brain tumors the average temperature of the whole head is elevated several degrees above the normal; and that the elevation of temperature is usually greatest at the station nearest the seat of the growth.

Constipation.—Constipation was recorded in eight cases. In some instances the bowels, after having been obstinately closed for days, would open involuntarily. In the terminal periods, involuntary evacuations both of the bowels and bladder often occur, but a real diarrhœa is rare.

Miscellaneous Points.—Severe *neuralgic* pains in the limbs were complained of by one of the cases of cerebellar tumor (Case 9). The other case of tumor of the cerebellum suffered from trigeminal neuralgia. One case of tumor of the pons Varolii had frequent and severe attacks of temporal and orbital neuralgia. Extreme trigeminal neuralgia, in association with the other symptoms of brain tumor, would seem to point to the pons or cerebellum as the probable seat of the growth. A study of the *reflexes* was unfortunately neglected in most of the cases. In Case 4, however, a carcinoma of the right motor zone, the left patellar reflex was diminished; while in Case 7, an occipito-parietal tumor of the left hemisphere, the patellar reflexes were a little exaggerated, and even slight ankle clonus could be brought out on the right side. The persistent *epistaxis* and tendency to *hemorrhage from the mucous membranes* were

interesting vaso-motor phenomena in the case of tumor situated in the left upper quarter of the pons (Case 11). Profuse *perspiration*, more marked on one side, was noted in Case 3. *Polyphagia* was present in one (Case 1). *Disturbances of respiration* were observed in several cases. In Case 6, a tubercular tumor of the right motor zone, with breaking down of the interior of the hemisphere, Cheyne-Stokes breathing occurred on the day of death. Extraordinary slowing of respiration occurred in Case 10, a tumor of the right middle cerebellar peduncle and cerebellar hemisphere, which caused irritation and softening of the floor of the fourth ventricle. *Hysterical manifestations* were noted in several instances, and are of not infrequent occurrence. Cases 1 and 3 exhibited *inflammatory, trophic, and anæsthetic phenomena* in the eyes. In both cases the growths were in the antero-frontal region, one extending to the base, and the other not involving the orbital surface. It is worthy of remark with reference to Case 1, that the tumor was in the *right* præfrontal lobe, and the *left* eye was attacked. *Smell* was observed to be defective in Cases 1 and 3. *Defective hearing* was noted in five cases. In Case 3, a tumor in front of the optic chiasm, hearing was defective in the left ear, the same side as the paralysis and other marked symptoms. Softening at the base was present in addition to the tumor. In Case 4 it was defective in the right ear, the tumor in this instance being situated in the right motor zone. The white matter of the brain on the inner side of the tumor was broken down. In Case 6 hearing was impaired in the left ear. The tumor in this case involved the posterior extremities of the first and second frontal and upper thirds of both ascending convolutions of the right side; and white matter of the parietal lobe, the posterior third of the lenticular nucleus, and the adjacent posterior portion of the internal capsule were softened. In Case 7, a tumor of

the left occipito-parietal region, hearing was defective in the right ear; and in Case 8, a tumor and abscess of the left temporo-sphenoidal lobe, it was also impaired in the right ear. These last three cases would seem to give some support to the localization of auditory centres in the temporo-parietal region.

The Local Diagnosis of Tumors of the Brain.—Niemeyer would hardly say to-day that "the 'brilliant diagnoses', where the precise localization of a tumor is fully confirmed by autopsy, are not usually due to acumen of the observer, but are cases of lucky diagnosis."

The locations of the tumors in the twelve cases may be summarized as follows :

Antero-parietal region	.	.	.	3 cases.
Rolandic	"	.	.	3 "
Parieto-occipital	"	.	.	1 case.
Temporo-sphenoidal lobe	.	.	.	1 "
Cerebellum	.	.	.	2 cases.
Pons Varolii	.	.	.	2 "

Tumors of the middle portion of the base of the brain, the region of origin of the various cranial nerves, can, of course, be diagnosticated with comparative ease by a study of the various forms of paralysis and spasm in the distribution of these nerves, in connection with other special and general symptoms. In several cases this region was more or less involved. Varieties of alternate hemiplegia are to be looked for, and also isolated or associated palsies of the oculo-motor, pathetic, facial, trigeminal, and other cranial nerves. In studying these palsies, it must be borne in mind that although the lesions producing them are intracranial, the paralysees themselves are peripheral.

Tumors of the Antero-Frontal Region.—Tumors of the præ-frontal lobes can be pretty certainly diagnosticated, partly by a study of the actual symptoms observed, and partly by

a process of exclusion. Headache of the usual type, vertigo, choked discs, inflammatory and trophic affections of the eyes, widely varying body-temperature, and high head-temperature are among the most positive manifestations. Mental slowness and uncertainty seem to be greater in these cases than in others. The absence of true paralysis and of anæsthesia is characteristic. Nystagmus and spasm in the muscles of the neck and forearm were present in one instance, but usually marked spasm is not to be expected. Vomiting would seem to be less frequent than in cases of tumor situated farther back.

Tumors of the Rolandic Region, or Motor Zone of the Cortex.—Tumors of the motor zone of the cerebral cortex, the region surrounding and extending for some distance on each side of the fissure of Rolando, can be diagnosticated with great positiveness. Three of the twelve cases were examples of tumors of this region, and in each case the diagnosis of the location of the growth was accurately made during life. Seguin¹ gives the following symptoms as characteristic of tumors in the motor zone of the hemispheres: Localized convulsions in peripheral muscles; equally localized paralysis of peripheral parts; neuro-retinitis or choked disk; localized headache. To these I would add pain elicited or increased by percussion of the head near the seat of the tumor, and elevated temperature of the head, particularly in the region corresponding to the position of the growth.

The spasmodic symptoms usually precede the paralysis in these cases. The spasm is often local and generally begins in the same part in different attacks,—in the fingers, or toes, or face of one side.

Tumors of the Parieto-Occipital Region.—Tumors of the

¹ On the Early Diagnosis of some Organic Diseases of the Nervous System, by E. C. Seguin, M.D., *N. Y. Medical Record*, Feb. 26, 1881.

parieto-occipital regions present some characteristic peculiarities. Case 8 was the only one in which the tumor was located in this region; but in several others the white matter of the parietal lobe was softened as the result of the obliteration of blood-vessels by the tumors. In general terms, I might say that hemi-anæsthesia, partial or complete, and impairment of sight and hearing, on the side opposite to the lesions, seemed to be the most constant peculiarities.

Tumor of the Temporo-Sphenoidal Lobe.—The single instance of tumor and abscess of the temporo-sphenoidal lobe (Case 8) was not very closely studied. Besides the headache, vomiting, stupidity, and constipation, the only symptoms which seemed characteristic were the partial anæsthesia and defective hearing on the side opposite to the lesion. Sight was impaired, but a comparative study of the condition of vision in the two eyes was not made.

Tumors of the Cerebellum.—The diagnosis of tumor of the cerebellum can be made with considerable certainty, although, as a rule, not much will be learned of the functions of the cerebellum through a study of such cases. The symptoms are, in large part, the result of pressure and irritation of adjacent regions—of the floor of the fourth ventricle, of some of the cranial nerves, etc.

In the two cases of tumor of the cerebellum given in the list, headache, vomiting, vertigo, mental dulness or slowness, general muscular weakness, and impaired sight, were present. Right partial hemiplegia and hemi-anæsthesia were observed in one case, but these symptoms were doubtless due to an area of softening found in the left optic thalamus, internal capsule, and lenticular nucleus. Hyperæsthesia and severe pains in the limbs were marked in one of the cases. In one vomiting was almost incessant for four weeks. Vertigo in this case was also intense; late in

her illness the patient could not raise herself from a recumbent position owing to the vomiting and vertigo. The cases were seen at too late a period to study carefully the peculiarities of the vertigo, and special disorders of movement. The slow rate of respiration in one case was quite remarkable.

In one of the cases internal hydrocephalus was present. According to S. Mackenzie, tumors of the cerebellum are very liable to be complicated by effusion into the ventricles of the brain, due either to pressure on the venæ magnæ Galeni, or to obliteration of the communication between the spinal and cerebral subarachnoidal spaces. (Ross on "Diseases of the Nervous System," volume ii, page 673.)

I agree with Nothnagel that a study of cases of atrophy of the cerebellum would be much more likely to throw light upon the functions of the brain than an investigation of cases of tumor and other lesions, which may cause functional disturbances of neighboring regions.

Tumor of Pons Varolii.—A large tumor of the pons Varolii can always be positively localized. Smaller growths may be localized with considerable definiteness in each of the four quarters of the pons. Alternate hemiplegia of the facial type was not present in either of the two cases of tumor of the pons, a fact which is explained by the comparatively small size and peculiar locations of the growths. In Case II, the tumor, one half inch in diameter, was distinctly limited to the left upper quarter of the pons. The chief symptoms diagnostic of location were right hemiparesis and ptosis; partial anæsthesia of right limbs and left side of face; and conjugate deviation of the eyes and rotation of the head to the right. The other usual symptoms of tumor, with the exception of vomiting, were also present. It would seem that tumors limited to one half of the upper portion of the body of the pons will cause con-

jugate deviation of the eyes and rotation of the head away from the side of the lesion. This symptom may be due to an involvement of the fibres of the middle cerebellar peduncles which pass through the pons. In Case 12, in addition to the headache, vomiting, vertigo, mental failure, constipation, and descending neuritis, the diagnostic signs were trigeminal neuralgia, and spasms most marked on the right side of the body.

(To be continued.)

EDITORIAL DEPARTMENT.

THE NEW DEPARTURE AT THE ROOSEVELT HOSPITAL.

At last one of the important general hospitals of New York has organized a continuous surgical service. This unquestionable advance has been brought about, at this late day, by a variety of causes, and we believe that it will mark an epoch in the history of medicine in this city.

For four or five years the time-honored system of rapidly rotating medical and surgical services, each physician or surgeon being on duty for two or three months, has been severely criticized in various ways.¹ This absurd system, which operated to the detriment of patients, which tended to demoralize the young *internes* of the hospitals, and which seriously crippled hospitals as centres for education, was perpetuated, we believe, mainly to allow a large number of physicians to enjoy the honorable title of physician or surgeon to a hospital, and to economize their time for private practice; and it is not surprising that the present beginning of reform should have been brought about by the persistent efforts of a small minority of the large number of the hospital physicians of New York. "To the victors belong the spoils," is sometimes a good saying, and we are glad that the new continuous service at the Roosevelt Hospital has come into the hands of one who by his pen and by his personal appeals has probably done more than any one else to bring about the change.

¹Consult Report of the Medical Board of the New York Hospital as to the Internal Administration of the Hospital, pamphlet, 1876. Editorials in ARCHIVES OF MEDICINE, vol. v, p. 177, and p. 291; vol. vi, p. 57.

One of the first practical attempts to improve the medical and surgical services of our hospitals was the simultaneous action of the governors of the New York Hospital and the trustees of the Roosevelt Hospital in 1878. This action consisted in the adoption of resolutions which provided that whenever by natural causes, such as death or resignation, vacancies occurred in the medical and surgical staffs of these institutions, these vacancies should not be filled until the number of each staff was reduced to three physicians and three surgeons: and, further, that in future, no newly appointed physician or surgeon could serve in both hospitals. Thus a blow was struck at the evil so prevalent in New York, of medical men holding appointments in two or three hospitals.

It is pleasant to add that a small majority of the medical staff of these two hospitals were heartily in favor of this movement, and urged their trustees and governors to take the decisive step.

At the Roosevelt Hospital, by the operation of these resolutions, the staff of attending surgeons was very soon reduced to two members, and the beginning of this year (1882) saw the entire surgical service of this important hospital under Dr. H. B. Sands and Dr. Erskine Mason. This division embraced a total of seventy-two beds, with an average occupancy of fifty patients. One would naturally suppose that a very satisfactory solution of the problem had been reached by the assignment of the two surgeons named, either to have the whole service for alternate periods of six months, or, better, by making two continuous services of thirty-six beds each. This latter plan would have been preferable, if complemented by the creation of a second staff of surgical *internes*, giving each surgeon a full corps of assistants. The extra expense entailed upon the hospital by the board and lodging of these gentlemen, would, in my opinion, have been compensated by the increased attention received by patients, and by the education of thrice the number of resident pupils to serve the community afterward. It is useless to speculate upon the manner in which this service might have been properly divided, since the

problem, which apparently presented some difficulties, was abruptly solved by the death of Dr. Mason, in April. In him the hospital lost a faithful and distinguished surgeon, and the profession one of its worthiest members.

By this event, Dr. Sands was left the only attending surgeon of the hospital, and the trustees were called on to decide upon the appointment of a successor to Dr. Mason. The medical board of the hospital, by a majority of one, resolved to recommend to the trustees the names of certain candidates for the vacant position. Three members of the medical board thought that the service should have two surgeons, and two members thought that it would prosper better under one head. This difference of opinion was a sincere one, and each side could bring good arguments in support of its view of the situation.

The trustees of the hospital were also much interested in this question, and when they received the list of candidates from the medical board, they referred these names and the whole matter to a committee of three. This committee, after mature deliberation, reported their conclusions in the shape of the following resolution, which was unanimously adopted by the trustees (seven out of nine being present), on May 30, 1882:

“Resolved, that Dr. Sands be the sole attending surgeon for a period of one year, and that he have one assistant, to be appointed by the board.”

Thus the Roosevelt Hospital assumed an advanced position in entrusting its entire surgical service to one head.

Is this extreme reaction from the former plan of broken services a desirable innovation? We doubt it.

It would seem desirable in a hospital to keep the door open to a wholesome competition, or rather emulation, between its higher medical officers. A single service, under one head, even though it be a most admirable one, can hardly help degenerating, or rather dropping into a stationary routine corresponding to the increasing conservatism of its chief. Besides, there are few surgeons who would not admit the advantage, in a practical and in a moral sense, of having at least one colleague of equal rank with whom to con-

sult and with whom to vie in attaining perfection in professional work. The saying, "It is not good for man to be alone," applies, it seems to me, with much force to such a question. Yet, as compared with the former state of affairs, the present organization of the surgical service at the Roosevelt Hospital constitutes a great advance, and all who know the gentleman upon whom has fallen the responsibility of managing this whole service must feel that he, if any one could, will succeed by personal devotion and professional skill in conducting it advantageously to the patients and to the hospital.

Still, we cannot but hope that before long, as the hospital is enlarged, another surgeon will be appointed, and two continuous surgical services, each with a full resident staff, organized. One hundred beds would be sufficient for two quite active surgical services or divisions. The results to be obtained from such an ultimate and permanent arrangement would be collaboration and emulation between the two surgeons, the thorough education of a larger number of young surgeons in the house staffs, the giving of more individual care to each patient, the elaboration of bedside notes and histories of cases, and the more systematic use of the material of the services for oral teaching, and for publication. A little reflection and analysis will show to any reasonable mind that all these objects, though some of them at first sight seem to be in favor of the surgeons, or of pupils, in reality and practically tend to benefit the sick and wounded in the community at large. We are perfectly willing to admit that a hospital has for its first and chief and last object the care of the sick; but how much greater is the usefulness (in this sense) of a hospital that becomes a centre of instruction for its medical officers and for pupils from without, thus increasing and perfecting the medical skill which is to be available to the community, and even to the whole country.

One clause in the resolution by which the trustees of Roosevelt Hospital created a single surgical service—that relating to the appointment of an "assistant"—is much more important than appears on its face. If there were two surgeons of equal rank, with the

understanding that both should never be absent (except a day or two) from the city at the same time, the use of the term "assistant," and the appointment to this position might hardly be worthy of comment.

Under the present circumstances the "assistant" will have to be the *alter ego* of Dr. Sands for several weeks probably, in charge of one of the most important surgical services in the country. He will have the care of many serious cases, will have to perform capital operations, will have the control, in a professional sense at any rate, of a staff of very intelligent *internes*, and yet he is expected to do all this without real rank, and, in the case of the present appointee, without sufficient professional standing. He is not Assistant Surgeon to Roosevelt Hospital, as he should be; and however talented and promising a man he may be (and we have good reasons for recognizing in the gentleman appointed the promise of all qualities necessary for the position), his name, his reputation, and his scientific contributions will not satisfy the demands of the profession and of the friends of Roosevelt Hospital when they ask who takes Dr. Sands' place; who has this splendid surgical service during Dr. Sands' unavoidable absence in the summer months?

We regret to say it, but we believe that in this is the chief weak point, if not a positively objectionable feature in the new departure at the Roosevelt Hospital. Through courtesy, and through a lack of appreciation of the true duties and responsibilities of the "assistant" named in the resolution of May 30th, the medical board and the trustees have made a mistake. The assistant surgeon of such a service should be almost if not quite the equal of its head in talent, experience, and years, because at any time the responsibility of the work may fall upon him, and because he is naturally a candidate for the position of surgeon should it become prematurely vacant.

Taking every thing into consideration, we think that the profession should look with hopeful satisfaction upon the experiment, and we trust that a progressive change in the same direction, though perhaps less extreme, will go forward in all our general hos-

pitals. Some of them are on the good road, and will almost inevitably reach a result within the next four or five years.

Trustees or governors of hospitals, for the sake of the patients in whose behalf they administer their trusts, and medical men for the sake of humanity and of progress in medical science, should not cease asking for continuous services.

E. C. SEGUIN.

NEW BOOKS AND INSTRUMENTS.

RECENT AMERICAN WORKS ON OPHTHALMOLOGY.

(1) **A Treatise on Diseases of the Eye.** By HENRY D. NOYES, A.M., M.D., etc. New York: Wm. Wood & Co., 1881. (No. 12 of Wood's Library of Standard Medical Authors.)

(2) **The Diagnosis and Treatment of Diseases of the Eye.** By HENRY W. WILLIAMS, A.M., MD., etc. Boston: Houghton, Mifflin, & Co., 1881.

(3) **A Manual on Diseases of the Eye and Ear.** By W. F. MITTENDORF, M.D., etc. New York: G. P. Putnam's Sons, 1881.

(4) **A Manual of Ophthalmic Practice.** By HENRY S. SCHELL, M.D., etc. Philadelphia: D. G. Brinton.

It is curious, to say the least, that four books upon ophthalmology should have so suddenly been brought before the medical public from purely American sources, especially when we remember that until the appearance of the present volumes nothing more pretentious than Williams' small book on "Diseases of the Eye" could be reckoned as an American work on this subject. The efforts of our ophthalmologists seem to have been directed toward the editing of English works, as witness Bull's Am. ed. of Soelberg Wells, Green's ed. of Brudenell Carter, and the Am. ed. of Nettleship; or toward the translation of text-books from German and French sources, as in the Am. ed. of Stellwag, or the translation of Schweigger's "Hand-book of Ophthalmology." Of the four works at present under consideration, three at least claim positions as text-books; the last, in the words of the author, "only states briefly the generally accepted principles of ophthalmology," while "the small size of the book restricts allusions to authorities, to the history of the science, or to any other subject

than the aforesaid principles and the treatment of the various conditions." It is a well-arranged, concise, practical manual, doing well what has frequently been done before, and making no pretensions to "filling a want long felt," etc.

The third upon the list, Mittendorf's manual, is rather pretentious for a manual, being of some 450 pages, and thoroughly equipped with ophthalmoscopic plates (from Liebreich and Wells), and plates showing diseased conditions of the membrana tympani from Politzer, as well as some illustrations from Sichel's Atlas, some of which are fairly well done, but others are execrable. The manual is a compilation, and from so comparatively young an author we ought not to expect any thing more. It evidences indefatigable industry on the part of its author, as well as a praiseworthy determination not to let his light be hid under a bushel. Its especial strength lies in the fact that it is double-barrelled, and in this respect the claim in the preface that it fills the long-felt want of a short, practical manual of the diseases of the eye and *ear* is valid, for, so far as the eye alone is concerned, the treatises of Williams, Lawson, Carter, the Am. ed. of Schweigger, *et al.*, have long since filled the gap. The combination of these two departments of practice (which are still so frequently, though so irrationally connected) in a single treatise, for the use of the economical student or country practitioner, is decidedly useful, and this, together with the judiciousness, thoroughness, and attention to detail which is everywhere apparent, has already carried the work to a second edition.

The main interest of the review centres around the first two works on the list. Their authors are representative men of different and rival cities, and though science is cosmopolitan we all know its practical application is apt to have local coloring. Both are men of large experience; one was, and the other is President of the American Ophthalmological Society, and their opinions upon mooted points of ophthalmology, of which there are not a few, have been looked for with interest by those who knew the books were in press. In general gracefulness of style, elegance of phraseology, and choice English, Williams' book has a decided advantage. Such innovations as "hit off the place" (Noyes, p. 72) are not found in it, while, on the other hand, it lacks the terseness of statement and systematic presentation of the different divisions of the subject which are so characteristic of all Noyes' scientific communications. It has been said that these books do not represent the American school of ophthalmology because

there is no such school, which is very true, for American ophthalmologists are peculiarly cosmopolitan by education, drawing most largely from German sources, but also freely from the English and French ; and in these days of free intercommunication and international congresses, the different "schools" are separated by lines which are yearly growing less distinct.

Nor can these books be called "model" text-books, though either one is a fair representative. Williams' is better for the student who does not care to be encumbered with nice anatomical or pathological points or with frequent reference to the literature of the subject, while Noyes' is better for the more thorough painstaking man.

It is true that science is not advanced by text-books but rather by monographs ; nevertheless monographs reach but a few, and many who are reached do not comprehend their bearings or importance ; and it is absolutely necessary that every few years some text-book by a representative man shall gather together the grains of wheat which these monographs have brought along with their load of chaff, and put them into shape to be appreciated and utilized by the student and general practitioner. This is what has been done by the works now under consideration.

Each has its especial strong points : Noyes dwelling with more detail upon muscular derangements, plastic operations about the lids, and affections of the nerve ; while Williams gives more attention to superficial inflammations, as may be seen by his division of conjunctivitis into fifteen varieties against six by Noyes.

Noyes seems to have followed Schweigger and others in dividing his work into two distinct parts, of which the first embraces the different methods of examination of the eye, the *general* nature and treatment of ocular diseases, the errors of refraction and accommodation, and diseases of the muscles ; while the second part takes up the diseases of the different tissues in the anatomical order. Williams, on the other hand, after a brief consideration of methods of examination and of general treatment, begins with the conjunctiva and goes through the whole series of tissues systematically, and then, abandoning system, goes on with anomalies of refraction and accommodation, muscular disorders, ophthalmitis, orbital diseases, and lachrymal and palpebral disorders.

Noyes' chapters on the general anatomy and physiology of the eye, as well as on the blood-vessels of the globe, all of which are omitted from the other work, are very useful in leading up to the subject, and rounding out the work without adding essentially to its size.

In the chapters on general treatment we find both rejecting the use of leeches in reducing inflammation, and condemning blisters and setons as of little or no use. Indeed Williams calls the latter "a relic of barber surgery," which is rather strong language for a method advocated by Critchett, of London, as of great use in obstinate ulceration of the cornea.

The chapters on the ophthalmoscope are full and excellent in both works, but it is singular to find no allusion to the light streak on the retinal vessels in Williams' description of the ophthalmoscopic picture.

The statement in the same section, that "the direct method of examination is only exceptionally used for purposes of exploration to discover pathological changes," may hold good for the latitude of Boston; but in New York it is, by most ophthalmologists, used in every case requiring ophthalmoscopic examination, and would be the one retained, if both could not be employed.

In the local treatment of phlyctenular conjunctivitis there is considerable discrepancy. Williams, rejecting calomel and atropine, relies upon pilocarpine (which remedy seems to have taken a strong hold upon his admiration) and very weak astringent collyria, while Noyes uses calomel, Pagenstecher's ointment, and in some cases atropine.

In gonorrhœal conjunctivitis Williams' treatment is not vigorous enough to meet the requirements of the New York plan. While agreed upon the advisability, in many cases, of canthotomy, there is a decided difference as to the subsequent course; Williams relying upon cleanliness and very mild astringents, Noyes pursuing a more vigorous course, of leeches, cold compresses, and nitrate of silver.

In the treatment of *ulcer of the cornea* Williams makes a decided innovation in advising against the use of atropia, and justifies himself by saying: "For years I accepted this treatment by atropia, with suitable auxiliaries, as being the best within our knowledge, and a great improvement upon the more violent measures of the olden time. But I could never satisfy myself that any sedative action was exercised by atropia or other mydriatics in affections of the cornea. On the contrary, the increased sensitiveness to light, caused by the wide expansion of the pupil, seemed to more than counterbalance any possible benefit, because the spasmodic closure thus excited in the lids, and their close friction over the ulcer, could have only a painful and harmful effect, tending to extend and deepen the ulcer, to favor in some measure a

forced infiltration of necrotic material from the ulcer between the layers of the cornea, and to increase the danger of a giving way of the cornea at its thin part and the protrusion of the iris through the perforation. This is no mere theory, but the conclusion reached after thoughtful and extended observation. The use of means calculated to intensify one of the most constant and characteristic symptoms of a disease, and in so doing to augment other morbid conditions, could scarcely be regarded as fulfilling the probable requirements for successful treatment. A substitution of remedies which relieved this urgent symptom appeared, therefore, to be a judicious as well as a scientific adaptation of means to an end, which was worthy of a practical trial. The discovery within a short time of the myotic qualities of Calabar bean and jaborandi, with their alkaloids, eserine and pilocarpine, by means of which we obtain the long-wished-for power of contracting the pupil at will, has placed at our disposal two invaluable remedies. My experience in careful trials of these myotics in corneal affections has been confirmed by that of other observers in Germany and elsewhere, some of them speaking of these as the remedies of the future in corneal affections; and it seems to be demonstrated that the advantages hoped for from their use are to be realized."

In the treatment of opacities of the cornea Williams advises against the use of local stimulants, believing that the improvement sometimes seen is a *post hoc*, and not a *propter hoc*. Noyes advises local stimulation in non-vascular or slightly vascular ulcerations.

As regards arresting staphyloma of the cornea by iridectomy, Williams is an unbeliever, saying it is of no avail. Noyes holds that in some cases a broad iridectomy will "reduce intra-ocular tension and confer a measure of sight."

The treatment of pannus by inoculation is regarded by Williams as unjustifiable, but from the way in which he speaks of it, it would seem that he has had no experience with it; while Noyes, who speaks from the vantage-ground of some successful cases, thinks that in cases of complete pannus in vigorous subjects the cornea may sometimes recover its clearness.

The removal of pterygium is conceded by both to be best effected by excision, but while Williams advises against any attempt to cover the raw surface of the sclera by conjunctival flaps, as militating against the certainty of cure, Noyes gives directions for making the flaps, which do not differ essentially from those advocated by Knapp some years ago. Williams' position, that

"all preparations of lead should be stricken from the list of remedies which may be used in the eye," seems to us the only safe position for a text-book to take; and the statement, that "usage gives currency and they may sometimes be used without harm," given in the other work, is weak and unworthy of the book.

The very important subject of iritis discloses little discrepancy of opinion except in the treatment of serous iritis, which Noyes does not make essentially different from that of the plastic form, while Williams says of it that "atropia is by no means to be regarded as the treatment, *par excellence*, to be used early and often as in the plastic variety." The latter considers a solution of the strength of two grains atropiæ sulph. to $\frac{3}{4}$ i of water sufficiently strong to rupture adhesions; while the former advises at least double that strength, and in New York even eight grains to the ounce is frequently used.

The chapters on cataract have an especial interest by reason of the debatable points upon which we are constantly seeking the results arrived at by our most experienced ophthalmologists.

Noyes would operate on *traumatic* cataract in one eye because of "gain in enlarging the field of vision, in the stronger mental impression, and because, notwithstanding no correcting glass was worn, a degree of stereoscopic vision was secured which the patients many times found of great advantage." Williams would not operate "because the eye operated on, having no accommodation, does not harmonize with the more perfect visual act in the opposite eye, and at times confuses instead of aiding it."

The latter would operate on both eyes at once in senile cataract, if both were ready; the former would not, preferring to give the second eye the advantage gained from the experience of the first operation. Anæsthesia by sulphuric ether is recommended in most cases by Williams with a fervor inspired by the sight of the statue in the public gardens commemorating an event alluded to by all Bostonians who write medical books. Noyes says the employment of anæsthesia "depends on the habit of the operator and the wishes of the patient." The use of corneal sutures to close the wound after extraction of the lens, which was first proposed about ten years ago by Williams, is still recommended by him, though it is certainly not in general use among ophthalmologists. Removal of soft lens matter by aspiration is condemned by both authorities.

As regards the best method of extraction for the majority of

cases, we find Williams rather non-committal. According to Noyes, the modern operation known as Graefe's modified linear extraction has quite supplanted the "flap method," while Williams evidently has a fond affection for the "flap method," which he performed so skilfully long before the new method was devised, and he draws attention to the fact that the development of secondary consequences—viz., separation of the retina, and irido-cyclitis—is beginning to be noticed in an alarmingly large number of cases.

The new operation of optico-ciliary neurotomy as a substitute for enucleation is more enthusiastically mentioned by Williams, who says, "there appears to be every reason for believing that it may replace the graver operation (enucleation) in a large number of the cases, at least, where enucleation has been performed as a preventive measure, before the second eye has become affected." Noyes says, "it has claims to consideration and is being extensively tested."

Of sclerotomy, the proposed substitute for iridectomy in glaucoma, Williams says: "It seems to be conceded that though sclerotomy offers a fair chance of success, iridectomy is most to be relied on for a certain and permanent curative effect, and it is, perhaps, sometimes useful for the relief of persistent pain in absolute glaucoma, where vision is hopelessly lost." Noyes says: "My own experience with it is small, and leaves me to favor iridectomy in critical cases. The efficacy of the proceeding is not fully established, although some have written strongly in its favor."

In the treatment of stricture of the lachrymal passage there is a decided difference of opinion, Noyes favoring thorough incision at once, followed by the use of *large* probes, even as large as 4 mm. in some cases. Williams, on the contrary, incises very cautiously, and uses probes of moderate size only.

The subject of color-blindness is lightly touched upon by Noyes, who does not seem to have taken any special interest in the investigations which have lately been made in this department, if one may judge from such inaccuracies as saying in Holmgren's test that the patient is requested to match *shades* of color, or saying that the proportion of women who are color-blind is 2 per cent. instead of $\frac{1}{4}$ of one per cent. Williams' chapter on the subject is full and accurate, except that he *understates* the proportion of the defect in females, placing it at $\frac{1}{40}$ of one per cent.

It is unfortunate that so excellent a work as that of Noyes should come to us in so mean a garb. The paper is poor, the wood-cuts

in many cases miserably done, and in short the whole execution is unworthy of the subject-matter, if we except the ophthalmoscopic plates, which are better than those in the Boston book, which, coming from the Riverside press, fully sustains the high reputation of its predecessors, and its perusal is not calculated to increase any of the disorders of which it treats. [S. B. ST. J.]

The Physician Himself and What He Should Add to the Strictly Scientific. By H. W. CATHELL, M.D., Late Professor of Pathology in the College of Physicians and Surgeons of Baltimore, Ex-President of the Medical and Surgical Society, Active Member of the Medical and Chirurgical Faculty of Maryland, Honorary Member of the Lincoln Philosophical Society, etc., etc. Baltimore: Cushings & Bailey, 1882, pp. 194.

"Professional tact and business sagacity," says the author of this valuable little work, "are as necessary to the physician as the mariner's compass is to the navigator. There are gentlemen in the ranks of our profession who are perfectly acquainted with the scientific aspects of medicine, and can tell you what to do for almost every ailment that afflicts humanity, who, nevertheless, after earnest trial, have never gotten either reputation or practice, because they lack professional tact and business sagacity; and there is nothing more pitiful than to see a worthy physician deficient in these qualities, waiting year after year for practice that never comes. * * * You will find that intellect, genius, temperance, correct personal habits, and other excellent qualities will all fail unless you add ambition, self-reliance, and aggressiveness to them." The book is in fact a mine of practical hints, commonplace it is true, yet valuable because they are commonplace. To the young practitioner it will prove a guide to correct business habits in many ways, and will put him upon his guard against multitudes of little obstacles that beset the pathway to success. To those who have been longer in the profession it will serve as a prompter to re-establish methods that have fallen into disuse from lack of time, negligence, and indifference. No one can read the book without finding some point which possesses a personal application impelling to more caution, or more vigorous action, as the case may be. We cannot repeat sound maxims too often, and though we may differ in opinion with the author in many particulars, such differences of themselves set us thinking upon details that are too frequently neglected. The difficulties of writing such a work can hardly be overestimated, and the author

is to be congratulated in having considered such a multitude of subjects in a manner above criticism ; on the other hand, the varied circumstances of the medical practitioner's life make certain of the author's maxims which may be applicable to one man, impracticable, or even ridiculous, for another. The intelligent reader will make allowance for this ; but then comes another class of statements, too numerous to consider in detail, which must meet with solid opposition from a practical or an ethical standpoint, and which by some will be considered not merely conservative but old-fogyish. The general tone of the work, however, indicates a bold conservatism, a happy blending of egoistic and altruistic notions. The author's style is terse, fearless, at times humorous.

Occasionally suggestions respecting treatment are introduced which seem a little foreign to the plan of the work. The following quotation is, to say the least, quaint : "Remember that the act of nursing not only supplies the babe with nourishment, but also communicates the mother's heat and electricity ; it is therefore well to have all *hand-fed babes* nestled to some one's bare breast at intervals of a few hours, in exact imitation of those that suck." A few other quotations will serve as better examples of the abundance of common-sense which pervades the volume, and the bold, clear style in which it is written. "When you hear of a doctor who wishes to be considered especially clear, or ahead of others, or extraordinarily fair in his opinion, boasting that he 'does not believe in drugs,' 'depends on nature,' etc., you can safely conclude that in his zeal to become a medical philosopher, he has doubtless lapsed in his *materia medica*, or overstated his credulity, or that his usefulness has run to seed."

"You are bound, as if by an oath, to use your best judgment for every one who puts himself under your care, but neither the code of ethics nor the code of honor prevents you from sailing as near to every popular breeze as truth and justice will allow."

"You will not have practised long before you will find that your welfare will not depend upon how much you book, but upon how much you collect, and that if you never insist upon the payment of your fees you never can separate the chaff from the wheat."

"Flexibility of manner, self-command, quick discernment, address, and ability to adapt yourself to the ever-changing phases of medical practice, are great necessities ; the lady in her boudoir and the hod-carrier, the beer-seller and the clergyman, the aged

and the young, should each find in you his ideal:—seek to become perfect in this power of adjusting yourself.”

The work is dedicated to Prof. Austin Flint, Sr., “in admiration of his various contributions to scientific medicine, and of his untiring devotion to the welfare of our profession.” [W. R. B.]

ORIGINAL OBSERVATIONS.

A CASE OF BULBAR APOPLEXY ; BILATERAL FACIAL PARALYSIS ; PARTIAL PARALYSIS OF THE TRIGEMINUS ; HEMIPLEGIA.

By HENRY J. BERKLEY, M.D.,

BALTIMORE, MD.

The patient, Mary B., æt. 40, a stout and vigorous, though intemperate woman, was stricken by paralysis in June, 1881.

Headache, severe and continued, was complained of for several days before the occurrence of the lesion.

When asked to mark out its locality, she lays her hand over the posterior region of the skull.

I requested the woman to describe to me how the seizure began. She answered : " I was standing upright when suddenly I felt drunk without having taken any liquor " ; in other words, her head swam, she felt dizzy, staggered, and fell.

As soon as she had somewhat recovered consciousness she perceived that the right side of the body was incapable of motion ; that she was paralyzed.

At the same time, she also remarked the hearing of the left ear had suddenly become bad.

A few days after the stroke she noticed that the eyesight of the left eye was growing dim ; soon the cornea became milky, gradually increasing until total blindness followed.

She tells me that she was completely paralyzed shortly after the attack, but that she never lost the feeling on either side of the body.

Stiffness of the paralyzed limbs commenced some weeks after the apoplexy, lasting between two and three months.

There has never been any embarrassment of respiration.

Physical Examination.—When the patient first came under my care, some four months after the attack, I found the right side paretic ; she could move the right leg slightly, and the arm could easily be raised to the face, only, however, with a very ataxic movement ; if I asked her to close the eyes, and touch the tip of her nose with the right index finger, she would invariably miss it, either putting the finger into the eye or upon the forehead.

The motions of the left hand and arm were natural.

On testing the strength, I found great loss of power in the right hand.

There was bilateral facial palsy, involving both superior and inferior facial muscles ; the features were stiff and immobile, the cheeks drooping, complete closure of the lips could not be effected, whistling was impossible, and the pronunciation of the labials *b*, *p*, *v* was incomplete. This facial condition (paralysis) was much more marked than that of the body.

The tongue is protruded to the right with an irregular, uncertain motion ; its upper surface is concave when put forth.

No atrophy is present in any of the muscles ; their faradic contractility is well preserved.

The facial muscles of both sides respond to faradism.

For several months after the date of the seizure (about five) there was a tonic spasm of the jaw muscles (temporal, masseter, etc.), so that it was impossible for the patient to open the teeth more than the one fourth of an inch.

Never has there been any dysphagia. The nails of both feet and hands are not in the slightest degree ridged ; on the paralyzed side they are observed to grow faster than on the sound.

There is incontinence both of urine and of fæces.

Tactile sensibility on the hemiplegic side is good everywhere, with the exception of a small portion of skin about the middle third of the forearm, on its flexor surface ; in this spot there was also slight analgesia.

On the right face sensibility is preserved, with likewise the exception of a little patch anæsthetic over the lower portion of the masseter and buccinator muscles.

On the left half of the face the whole of the region supplied by the supra-orbital branch of the trigeminus is totally analgesic and anæsthetic ; nowhere else is there the slightest diminution of sensation.

Tactile sensibility on both sides of the tongue is good.

The special senses were very carefully examined.

Taste and smell were intact ; bitters, salts, and aromatics being recognized without difficulty.

Auditory examination, by means of a watch, showed hearing to be totally extinct in the left ear, both by contact with the cranial bones and through the medium of the air, while in the right the function was normal,

Speech is slow, jerky, and drawling, but there is no aphasia.

The left eye presents an exceedingly interesting condition ; as before mentioned, shortly after the apoplectic stroke, the cornea became cloudy, thickened, and fleshy. At the time I first saw it the pannus protruded more than the one fourth of an inch beyond the lids, which were separated by the growing mass.

Apparently the protrusion of the eyeball is entirely due to the hypertrophied cornea, for on measuring the two sclera as near as possible to the corneal margins and comparing, there seems to be no appreciable difference in their level.

Contrary to the usual history, the cornea did not rupture.

During the last three months of life the cornea atrophied, the vascular injection lessened, and before death its apex sank below the level of the sclero-corneal margin.

Sensation in the diseased cornea is almost entirely abolished.

There is increased secretion of lachrymal fluid, the eye weeping continuously.

The movements of all the ocular muscles are perfect, nor is there any ptosis.

About the same period that the hypertrophied cornea began to subside, the spasm of the left jaw muscles, together with the paralyzed right facial muscles, ameliorated greatly, the woman regaining almost perfect control over the right face, and was enabled to open the mouth nearly to its full extent.

At times flushes of redness and heat, accompanied by slight tingling, will pass over the limbs and trunk of the right side ; indeed, the whole hemiplegic side has a more reddened hue than the sound.

As far as I can judge there is no loss of intellectual power ; the woman herself thinks her memory to be quite as good as ever it was.

There is some emotional disturbance, the patient laughing or crying at words or acts that would not be noticed by a healthy person.

The urine was only examined once ; it contained no albumen, nor has there ever been any polyuria.

Some two weeks before the end, bed-sores formed for the first time during her illness, commencing upon the small of the back, then over the sacrum. At the time of death the integument had sloughed to a considerable extent.

Etiology.—Although undoubtedly a woman of loose morals, no symptoms of syphilis could be detected on the most patient examination ; nor was there any abnormality in the heart's sounds, the rhythm being perfectly regular. Indeed, the only probable cause that could be obtained was that she had been a hard drinker, and I think that alcohol, most probably, was the cause of the arteritis that led to the formation of miliary aneurisms, the rupture of one of them giving rise to the hemorrhagic *foyer*.

Death, May 21, 1882, from gradual asthenia.

A diagnosis of hemorrhage into the bulb was made by Professor F. T. Miles, of Baltimore.

I owe the privilege of an autopsy to Dr. H. Woods, of Baltimore, the case having passed into his hands before death.

Necropsy, 28 hours after death.—No measurable difference could be found in the size of the extremities when compared.

The eye was completely atrophied, the cornea sunk, the sclera thinned ; even the lens had been absorbed.

The heart was small and very fatty ; the valves, aortic and mitral, were perfect in their junction, but were, however, very slightly thickened, with here and there an atheromatous patch, the degeneration not being sufficient to cause a murmur during life ; atheroma was also seen in the aorta.

In the abdomen the liver and kidneys were considerably diseased.

The brain : The skull was of great thickness ; the *diplœ* large, its meshes open, constituting by itself the greater part of the bone.

The dura mater was of normal aspect, not adherent to the cranial bones, nor to the meninges.

The lateral sinuses contained dark fluid blood.

The arachnoid presented that peculiar thickening so often found in atrophied brains (alcoholism).

The pia mater was opaque, but not adherent to the cortex.

Between one and two ounces of clear serum had collected in the arachnoid space, mostly at the base.

The vessels, basilar and carotids, were strewn with hard, white plaques of atheroma ; in several places the calibre of the arteries was very considerably reduced in size.

On the basilar artery, at the inferior margin of the pons Varolii, a fusiform aneurism of the size of a bean was found.

The seventh pair of the left side was seen to be slightly smaller and of softer consistence than the nerve on the opposite side. No departure from the normal was found in the optic nerves.

The hemispheres were atrophied considerably, not nearly filling the cranial cavity.

The convolutions were of simple arrangement, and seemed slightly flattened; the sulci did not attain any great depth between the convolutions.

The brain tissue was very soft, more than could be accounted for by *post-mortem* changes, but there was no localized degeneration of any kind on the cortex. Section of the white and gray substance revealed nothing abnormal; the ganglia exhibited their usual gray color; the internal capsule ran its regular course; everywhere the different parts were normal.

The lateral ventricles were dilated, their ependyma thickened; both ventricles contained a quantity of serum.

The aqueduct of Sylvius was slightly dilated.

The cerebellum was injected, probably *post mortem*, but was otherwise normal.

The bulb: On the floor of the 4th ventricle, on the left half, just above and to the outer side of the eminentia teres, was a small depression of irregular shape, slightly yellowish in color.

Transverse section disclosed an old hemorrhagic focus, of stellate form, extending from a point 5 mm. above the lower margin of the pons, 5 mm. internally to the floor of the ventricle, reaching upward nearly to the superior border of the pons, progressively growing smaller as it ascended. The centre of the lesion lay anterior to the genu nervi facialis. From about this point two arms extended from the focus far forward, nearly reaching to the border of the superficial transverse fibres of the pons. Nowhere did it approach the median line. Only at the point already mentioned did the focus touch the floor of the ventricle.

In the right half of the bulb, at a point slightly anterior to the nucleus of the facial nerve, was another focus of the size of a No. 5 bird-shot, yellowish in color, and apparently about the same age as the larger lesion.

Microscopic examination.—On examination of the smaller arteries, particularly those of the anterior perforated space, I found several miliary aneurisms.

The walls of the vessels showed the lesions of chronic arteritis.

Sections of the hardened cord, stained with carmine, showed a descending degeneration of the crossed pyramidal tract of the right half of the organ, and a corresponding sclerosis of the direct tract on the left.

The tissue was less densely sclerosed than is found in old cases of absolute paralysis, many more of the axes-cylinder seemingly being intact.

Apoplexy of the medulla, in which the patient survives for any length of time, is of comparatively rare occurrence. In a table collected by Gintrac¹ of 560 cases of cerebral hemorrhage in which there was a single clot, 76 were in the pons Varolii and crura cerebri, while only two were in the medulla proper. Hemorrhage is very frequent in aneurisms of the basilar artery and its branches. According to Lebert (*Berl. klin. Wochr.*, 1866): "Among 31 cases of aneurisms of the base, vascular ruptures and hemorrhages occurred 17 times into the parenchyma of the pons and medulla oblongata."



FIG. 1.—Oblique view of medulla and pons; 1, transverse section of surface of medulla; 2, transverse section of cerebellar peduncles; 3, cut crura cerebri; 4, corpora quadrigemina; 5, cicatricial depression in floor of fourth ventricle, immediately over the hemorrhage, marked by *

FIG. 2.—Transverse section of the pons just below exit of fifth nerve, showing the site of the hemorrhage within the pons.

[These diagrams have been drawn from the specimens. The reader who is specially interested in studying the localization and relation of the lesion more minutely can do so by projecting the site of the hemorrhage upon the various plates and diagrams of the medulla oblongata in Ziemssen's *Cyclopædia*, Am. ed., vol. xiii, pp. 838-844.—EDITOR.]

¹ Cited by Hammond, "Diseases of Nervous System."

Echeverria records a case of aneurism of both vertebrals, near their junction to form the basilar; the tumor on the right compressed the roots of glosso-pharyngeal and hypoglossus, which (tumor) occasioned during life dysphagia and paralysis of the tongue. Death sudden from hæmorrhachis.

During the past winter I met with another aneurism of the basilar, in about the same locality as in the present case; the artery also presented the anomaly of being formed by the junction of the vertebrals at the inferior border of the foramen magnum. Death resulted in this case from rupture of the external arteries of the lenticular ganglion.

The most difficult symptom to be understood in this case is the bilateral facial paralysis. To account for it we shall have to accept one of two hypotheses: first, that the second lesion in the right side of the medulla caused it; at the beginning having an additional influence by pressure upon the remaining sound portions of the nucleus, and when, in the course of time, by gradual absorption, permitted more complete motor neurization of the remaining intact fibres of the facial, then the muscles partially regained their former power. Otherwise, we must follow the descriptions of Bastian and Hammond, viz.: "that injuries of the upper part of one lateral region of the pons produce a hemiplegia of the opposite side of the body, accompanied by facial paralysis of the same side as the hemiplegia." In other words, if the lesion happen above the decussation of the encephalic fibres, the paralysis will be on the opposite side for all parts of the body.

Gowers, in the January issue of *Brain*, of 1880, mentions the frequency of bilateral symptoms of paralysis of the cranial nerves in the diseases of the pons Varolii.

Following Vulpian (*Essai sur l'origine de plusieurs paires de nerfs crâniens*), the decussation of the roots of the facial "occurs in the mesial line of the medulla oblongata, at the junction of the two nuclei of origin."

In the present subject the long axis of the lesion ran in the long diameter of the bulb, and possibly, in addition to destroying the lower nuclei, it may also have intersected the fibres coming from the cerebrum.

The partial involvement in paralysis of parts of the motor and sensory branches of the trigeminus is one of the most important circumstances of the case, and may be a valuable addition to localization in the anatomo-pathology of the bulb.

But first to recapitulate: There was absolute anæsthesia and

analgesia of the supra-orbital division of the ophthalmic ; perfect sensibility in the superior maxillary, malar, and mental branches ; and a tonic spasm, lasting several months, of those muscles, principally the masseter and temporal, supplied by the motor division of the fifth ; all on the left side, the right not participating.

I do not think it would be carrying a theory too far, to say that here, not judging from mere hypotheses, but from actual examination of the locality of the hemorrhage, drawing my authority for the location of the nuclei from Erb, we have destruction of the superior sensitive, together with compression of the motor nucleus of the fifth.

Now, from this fact, it is a very plausible hypothesis to consider that this superior nucleus is the spot from which alone are derived the fibres that convey sensation from that part of the face, corresponding to the distribution of the terminal filaments of the ophthalmic branch of the trigeminus, and that the inferior and larger nucleus supplies the fibres to the inferior regions of the face. The non-participation of this latter nerve centre can only be explained by the supposition that the inferior nucleus lies more in the postero-lateral portion of the bulb than it is usually considered to be, that portion not having been destroyed for 5 mm. from its outer margin.

Regarding the trophic disturbances following the destruction of the ophthalmic branch of the trigeminus, there is yet no definite settlement of the question as to whether the result is from the lesion of the nerve or from mechanical irritation of the exposed cornea.

The numerous experiments of Snellen, Meissner, Schiff, and others all lead to different results.

Samuel,¹ "after having produced a violent irritation of the Gasserian ganglion in rabbits by means of the induced current, observed the development of acute ophthalmia, accompanied by hyperæsthesia of the eye."

"In rabbits in whom the trigeminus has been cut within the cranial cavity, ulcerations readily occur upon the nose, lips, palate, and sometimes upon the tongue." (Rosenthal.)

These parts being habitually exposed to irritation, it cannot be deduced in favor of the theory that the keratitis is due to mechanical lesion of the exposed surface.

Samuel (*Die trophischen Nerven*, 1860), cited by Rosenthal. *Diseases of the Nervous System*. American edition.

Bock and Friedrich¹ have seen "cases in which ophthalmia developed in persons (suffering from tumor) despite the preservation of sensibility."

Weir Mitchell records a case (injuries to nerves) of a gun-shot wound of the ophthalmic branch of the fifth, in which, in less than three days from the time of the injury, the cornea became "dull, insensible, and slightly clouded." Surely this is not time for any external irritation to produce such results.

According to Charcot and Friedrich,² "the trophic disturbances of the eye are caused by an inflammatory irritation (descending neuritis)."

"In an experiment performed by Buettner,³ after multiple incisions into the Gasserian ganglion, and a purulent ophthalmia of rapid course (despite the application of protecting apparatus), the ganglion was found in a condition of traumatic inflammation, extending into the ophthalmic branch."

On the first examination of the focus it is difficult to conceive why the nucleus of the abducens was not destroyed. By more careful investigation the hemorrhage is found not to extend within 3 mm. of the eminentia teres, immediately within which lies the nucleus of the abducens (Erb), while at a greater depth, coming within the limits of the lesion, is the nucleus of the facialis, which was totally destroyed.

Scrutiny of the lesion revealed all that tract of the medulla anterior to the striæ acusticæ, destroyed, involving the "inner" and "outer" nuclei of the acoustic of Huguenin.⁴ This destruction I believe fully sufficient to cause the total auditory anæsthesia, even though the remaining nuclei of the acoustic were not involved.

There can be little doubt that the vaso-motor centres were injured, perhaps primarily in the medulla, then secondarily in the lateral columns and gray horns of the cord. The increased redness, vascular dilatation, etc., of the hemiplegic side showed that the vaso-motor contraction of the arteries was destroyed or lessened.

Owsjannikow and Dittmar have defined the vaso-motor centre in the medulla of the rabbit "as lying between the corpora quadrigemina and calamus scriptorius, somewhat removed from the middle line, in the lateral columnar tract of the formatio reticularis.

¹ Bock and Friedrich, cited by Rosenthal, *l. c.*, p. 485.

² Charcot and Friedrich, cited by Rosenthal, *l. c.*, p. 485.

³ Buettner, Rosenthal, *l. c.*

⁴ Huguenin, "Allgemeine Pathologie der Krankheiten des Nervensystems."

“The nerves for vaso-motor contraction which leave it are, said to run through the lateral columns to the anterior nerve roots.” (Dittmar.¹)

Even in man this demonstration would account for the vaso-motor lesions in this subject, for the tract destroyed by the hemorrhage corresponds to the location described by Dittmar in the rabbit.

The hemiparesis was owing to the circumstance that an arm from the focus intersects the pyramidal tract at its extreme outer edge, as can be readily seen in several of the sections, especially those of the middle portion of the pons.

Rigidity of the paralyzed limbs is very frequently encountered in bulbar apoplexies. (Bastian.)

It is a question whether the anæsthetic spots on the right side, on the arm and face, have any significance whatever; they did not correspond to the distribution of any special nerve, and more probably belong to some mere local cause, such spots being not infrequently met with when the cutaneous surfaces are examined with care.

¹Owsjannikow and Dittmar, cited by Erb, Ziemssen's *Cyclopædia*, vol. xiii, p. 861.

ARCHIVES OF MEDICINE.

Original Articles.

TUBERCULAR TUMORS OF THE WINDPIPE. TUBERCULOSIS OF THE LARYNGEAL MUSCLES. A CONTRIBUTION TO THE PATHOLOGICAL HISTOLOGY OF LARYNGO-TRACHEAL PHTHISIS.

By JOHN N. MACKENZIE, M.D.,
BALTIMORE.

I.—TUBERCULAR TUMORS OF THE WINDPIPE.

THE larynx of the consumptive is the seat of various forms of neoplasm which are pathologically separable into three distinct groups.

1. *Granular hyperplasiæ*.—This is the most common variety of outgrowth, and consists of an aggregation of small grayish or pinkish granulations, which fringe the edges or deck the base of tubercular ulceration. They are rarely, if ever, met with as the result of the simple catarrhal or diphtheritic¹ changes which are found in phthisis of the windpipe, and are therefore of considerable value in differential macroscopic diagnosis. They occur in all portions of the larynx, their seat depending on the location of the ulcerative process, and are sometimes so abundant as to cause stenosis and necessitate tracheotomy. Anatomically allied to granulation tissue, they may be regarded as the representatives of a conservative process—as a *natural step*

¹ On diphtheritic ulceration, see papers by the author in the *Monatsschrift für Ohrenheilkunde*, No. 9, Berlin, 1881, and the "Transactions of the Med.-Chir. Faculty of Maryland," 1882.

toward cicatrization. If sections be made through a tubercular ulcer of the larynx or trachea, and its surrounding granulations, a more or less clearly defined zone of hyperplastic inflammation of the connective tissue will be discovered, which isolates the degenerate tubercle from the healthy tissues in its immediate vicinity. The vegetations under discussion are the result of this protective inflammatory process, and consist, histologically, of a mass of newly formed connective-tissue cells and nuclei, in which enlarged, tortuous capillary vessels are sometimes developed.

2. *Papillomatous excrescences.*—Of less common occurrence than the preceding, the members of the second group are closely allied to simple laryngeal papillomata, and, like them, owe their origin to chronic vascular congestion of the mucous membrane. Their occasional presence, however, in the midst of pronounced pallor of the larynx, would lead to the belief that they sometimes result from the irritation of an opposite pathological condition, viz.: the peculiar anæmia of the laryngeal membrane, which is so often the herald of the tubercular process. Warty, acuminate, or leaf-like in form, and varying in color from a pale grayish to a pronounced reddish hue, they spring, as a rule, from the posterior laryngeal wall, and in this situation sometimes attain to a considerable size, and are readily mistaken for true laryngeal papillomata. They occur also on the epiglottis and below the anterior commissure of the vocal cords. In structure they are the histological analogues of the laryngeal papilloma, and occur, as a rule, independently of well-marked tubercular lesions. These growths often ulcerate and assume an apparently malignant form, but I am unaware of any microscopical evidence of their conversion into tubercular products. Stoerk,¹ who has carefully studied the development of these neoplasms, maintains

¹ *Klinik d. Krankheiten des Kehlkopfes*, etc., Stuttgart, 1880, S. 282.

that their presence in the interarytenoid fold is an infallible sign of incipient tuberculosis, and, following Rokitansky, regards them as the result of the indurative proliferation of the connective tissue, which occurs in the course of chronic tubercular disease of the mucous membrane in the neighborhood of the arytenoid cartilages.

3. *Tubercular tumors*.—The two above-described varieties of neoplasm contain no histological elements by which they may be differentiated from simple granulation or papillary vegetations. The third group, on the other hand, includes solitary tumors of the windpipe, which are composed, histologically, of a mass of closely aggregated miliary tubercular nodules, and which occur independently of infiltration and ulceration of the mucous membrane. I desire to call especial attention to these growths, as they are not described in works on laryngeal pathology, and I am unaware of a single recorded case of laryngeal neoplasm whose tubercular nature has been demonstrated by the microscope.¹ The two following, which came accidentally under my observation in the Rudolph Hospital in Vienna, are therefore worthy of publication. The *post-mortem* examinations were made by Dr. Hans Chiari, to whom I am indebted for the specimens, and the organs handed to me for microscopic investigation.

1. *Tubercular tumor of the trachea*.—This was taken from the body of a man who had died in the hospital from carcinoma of the stomach. Secondary cancerous deposits were present in the liver, kidneys, spleen, and other organs. The lungs, however, contained tubercular cavities. The pharynx, larynx, and trachea were free from inflammation and ulceration. The bronchial and retro-tracheal glands

¹ It is obvious that neoplasms of various kinds may be found in the air-passages of phthisical persons, which have no connection whatsoever with the tubercular process. The cases recorded by Andral *Clinique médicale*, t. ii, p. 195, (and *Spillan's Transl.*, Lond., 1836, p. 489), Trousseau "Laryngeal Phthisis," Phila., 1839, p. 31, and others, evidently belong to this category.

were enlarged, tumefied, and caseous. In the membranous posterior wall, at its junction with the cartilaginous framework of the trachea, about $1\frac{1}{2}$ cm. above the bifurcation, was a well-defined circumscribed tumor about the size of a small bean, its long axis parallel with that of the trachea, and of a uniformly even, smooth appearance. It was covered by the mucous membrane of the trachea, and was dense in consistence, giving to the touch the sensation of a hard cancerous nodule, for which, indeed, it was mistaken. A similar growth was found in the pericardium.

The microscope revealed a picture for which I was not prepared. It showed, namely, that the tumor, which seemed to have its origin in the submucous connective tissue, consisted, in the main, of an aggregation of distinct tubercular nodules set in a more or less well-marked vascular net-work of hypertrophied connective tissue. The majority of the tubercles lay in the deeper portions of the mucous membrane and in the submucous tissue; a few were more superficial, lying under the epithelium. They exhibited all grades of degenerative change; in some caseation was so far advanced that nothing remained but the cellular wall. Between the individual nodules the connective tissue was hypertrophied and the seat of a moderate amount of round-cell infiltration which had invaded the glandular follicles in its vicinity. The tissues of the trachea in the immediate neighborhood of the growth presented no remarkable change. *The nodule in the pericardium showed the same histological structure that was found in the tracheal neoplasm.* All the other diseased organs were shown by the microscope to be cancerous.

2. *Tubercular growth of the vestibulum laryngis.*—The subject from whom this specimen was taken died outside of the hospital, of pulmonary phthisis. The whole upper compartment of the larynx, including the epiglottis, ary-

epiglottic folds and ventricular bands, presented a remarkable mammillary or granular appearance, due to the presence of small, uniformly smooth, dense, moderately hard nodular growths, which lay beneath the mucous membrane. The nodules were about the size of a split pea, each merging into its neighbors, so as to form one continuous growth. This process ceased abruptly on either side, at the free border of the ventricular band. There was no trace of ulceration in pharynx, larynx, or trachea.

Microscopical examination of numerous sections of the growth showed it to be of the same nature as the above-described neoplasm in the trachea. Like it, it seemed to have invaded principally the submucous tissue, and, like it, consisted of a mass of closely aggregated miliary tubercles in all stages of degeneration. The nodules in the deeper portions were further advanced in caseation than those which were more superficial. The growth was less dense in consistence than the one observed in the trachea, and the connective tissue between the nodules had undergone a less degree of hypertrophic change. The tubercles lay in close proximity to the blood-vessels, one or two of which were compressed by the new formation. The remaining portions of the larynx were healthy. The lungs contained tubercular cavities. There were tubercular ulcers in the intestine, and the basilar surface of the brain was covered with miliary granulations.

As intimated above, these cases are probably unique. Such tumors doubtless have a similar origin to the so-called "metastases" in the laryngeal mucous membrane which take their departure from old tubercular disease of other organs, as the kidney (Köhhorn¹) and bronchial glands (Lennox Browne²).

¹ *Berliner klin. Woch.*, 1876, Nos. 3 and 4.

² *Med. Times & Gaz.*, 1876, i, p. 456. (Eppinger: "Pathologische Anatomie des Larynx u. der Trachea." Berlin, 1880, S. 155.)

II.—TUBERCULOSIS OF THE LARYNGEAL MUSCLES.

The hoarseness without palpable *intra-vitam* or *post-mortem* changes in the larynx, so often met with in phthisical patients, led Fraenkel,¹ in 1877, to examine the laryngeal muscles as a possible seat of degenerative processes which might explain the alterations in the voice. In twelve larynges, some apparently healthy, others presenting ulceration, he found constant anatomical changes, which consisted essentially in an atrophy of the striated muscles. The connective tissue between the primitive bundles was markedly infiltrated with cells, and the nuclei of the muscles so increased as to destroy apparently the muscular fibres by pressure of the new-formed cells.

Several years later, Posadsky,² of St. Petersburg, confirmed the observations of Fraenkel, and showed that the same degenerative changes took place also in the muscles of the upper and lower extremities, the intercostals, and the diaphragm. The muscles were pale and friable, and the microscope showed granular degeneration of the muscular fibres. In many cases there was disappearance of the transverse striation, and the fibres were notably narrowed, and some empty sarcolemma sheaths were seen. The interstitial connective-tissue changes described by Fraenkel were not discovered by Posadsky.

Heinze,³ who has carefully studied the pathological anatomy of laryngeal phthisis, is unable to confirm the results arrived at by Fraenkel, and thinks that tubercular infiltration of the tissues is sufficient to explain the anomaly in the voice. This observer has furthermore discovered tubercle in the muscular tissue of the deeper laryngeal structures, an occurrence, according to him, of extreme rarity. Out of

¹ "Ueber die patholog. Veränderungen d. Kehlkopfmusculatur bei Phthisis-kern." *Virchow's Archiv*, lxxi, 3, 1877.

² *Vide*, *London Lancet*, 1881, vol. i, p. 149.

³ "Die Kehlkopfschwindsucht," Leipzig, 1879, S. 69.

fifty carefully examined cases, tubercle of the muscles, unconnected with general tubercular infiltration of the larynx, was found only twice.

The apparent rarity, therefore, of miliary tubercular deposit in the muscular tissue of the larynx induces me to record the microscopical appearances observed in a case which I have recently had the opportunity to examine.

The specimen was taken from the body of a man who had died in an advanced stage of pulmonary phthisis. Cavities were present in both lungs. The epiglottis was the seat of several small ulcerated spots, and there was a large ulcer involving the whole of the left ventricular band, the anterior portion of the corresponding ventricle and vocal cord. The microscopical examination of the ulcer and the portions of the larynx immediately adjoining, presented the usual appearances of tubercular infiltration and ulceration.

In order to determine the changes in the muscular tissue, sections were made through the arytenoid cartilage and the muscles attached to its base. The histological changes consisted (1) in the deposit of distinct miliary tubercles in the substance of and between the muscular fibres, and (2) in alterations in the fibres themselves.

1. In the deeper portions, near the insertion of the muscular fibres into the arytenoid cartilages, were found several large, well-marked, distinctly circumscribed tubercles embedded in the muscular tissue, and wholly unconnected with the tubercular infiltration of the mucous membrane which clothed that portion of the larynx. They lay in the centre of the muscle, between the individual fasciculi, by which they were completely surrounded and which they had forced apart, so that at the situation of the tubercle the muscular bundle presented under the microscope an ampuliform dilatation. The outer fasciculi surrounded the tubercle completely, whilst the central ones passed appar-

ently through its substance, or ended abruptly at its margin in a mass of small round cells. The individual fasciculi were in some places torn completely asunder; in one place, absorbed by the pressure of an outlying tubercle. The tubercles themselves presented the typical appearance of these neoplasms, each containing several giant cells. In one, caseation had produced almost complete destruction of its central portion. In the immediate neighborhood and between the individual fasciculi, in some instances forcing them apart, was an abundant, high-grade round-cell infiltration. The cells were observed, on cross-section, to completely surround the separate fasciculi; at some points they had so invaded the muscular tissue as to leave little of its original structure visible.

2. The most noticeable change in the primitive bundles themselves was an increase in the number and size of the muscle cells. Those bodies were generally enlarged, sometimes irregular in outline, and filled with a granular matter; in others the nuclei were plainly visible. At some situations the muscular tissue showed no well-defined pathological change, beyond, perhaps, a slight increase in number of the nuclei; whilst at others, and especially was this the case in the vicinity of the tubercles, the striation was indistinct, the fibres were broken across, so that the sarcolemma sheath became visible, and in some spots the muscular tissue itself was converted into a granular detritus.

These appearances add little to what has already been observed by Heinze; but as corroborative evidence of the accuracy of his observation, they may prove of some interest to those whose studies have led them into this field of pathological research. As to the import of the granulo-fatty degenerative changes in the muscular fibres, I am inclined to regard them as the result of disturbances of nutrition induced by chronic inflammation, rather than as an integral part of the tubercular process.

A CONTRIBUTION TO THE CARDIO-SPHYGMOGRAPHY OF TRICUSPID LESIONS.

By A. T. KEYT, M.D.,

CINCINNATI, OHIO.

OBVIOUSLY the indications by the graphic method of lesions affecting the right auriculo-ventricular valve must be largely, if not wholly, negative, nevertheless their importance is sufficiently manifest. The value of negative evidence in diagnosis is well understood. Simultaneous cardio-arterial traces in left-side valvular disease give positive indications. Marked aortic stenosis, heavy aortic valves, mitral regurgitation, and aortic regurgitation, cannot exist without distinctive phenomena which express themselves through cardio-sphygmography as diagnostic signs of the respective conditions. In right-side valvular disease such graphic signs are not obtained. Hence, when the question arises as to which side of the heart is the seat of the valvular trouble, the graphic records come in to decide.

Especially it is known how difficult it is by ordinary methods of examination to distinguish between mitral and tricuspid regurgitation. Indeed, it is doubtful whether tricuspid regurgitant lesion has ever been positively recognized during life. Cardiac murmur, so much relied upon in the diagnosis of mitral lesion, is of little aid in the diagnosis of tricuspid lesion; nor are there any physical signs or

general symptoms by which the latter can with any degree of certainty be differentiated from the former. The two following cases are in evidence and illustration of the important diagnostic aids cardio-sphygmography may afford toward the recognition of tricuspid lesion.

CASE 1.—Mrs. Smith, aged 59 years, came first under my care in March, 1880. She was suffering from dyspnœa, cough, and general dropsy, which were found to be dependent upon organic cardiac disease. There were evidences of considerable enlargement of the heart, and a loud systolic murmur was audible over the cardiac region, plainly emphasized at the left apex, well heard to the left and in the back. The second sound of the heart was clear, and accentuated at the pulmonic site. Heart's action at times irregular; pulse somewhat accelerated, sufficiently ample; no albumen in urine. She continued under my observation without material change of the physical signs, and died, from failure of the circulation, May 8, 1881.

Simultaneous cardio-sphygmograms were obtained May 24, 1880, and February 12, 1881, of which figs. 1 and 2 are examples.

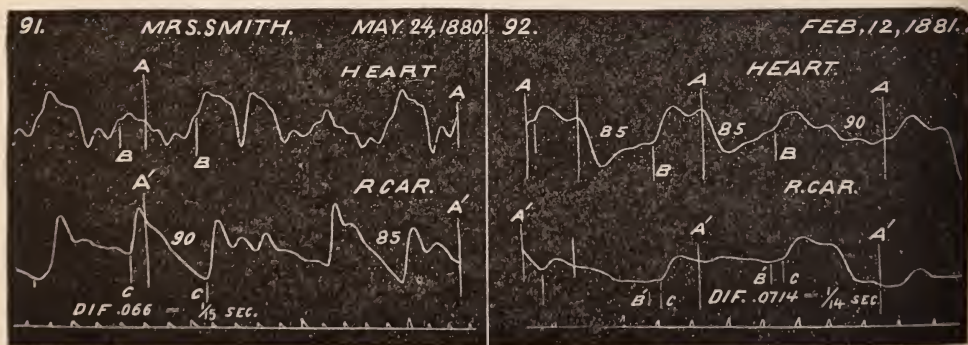


FIG. 1.

FIG. 2.

The *post mortem*, concerning only the thorax, was conducted by Dr. A. B. Isham in the presence, and with the assistance, of Dr. H. T. Lowry and myself. The pericardium contained a moderate amount of fluid. The heart was generally enlarged. Right

cavities and great veins distended with blood, and the former contained firm fibrinous clots. Walls of right ventricle and right auricle greatly hypertrophied—dilated and thickened,—the former measuring one half inch in thickness. Venæ cavæ dilated. Pulmonary valves normal. Tricuspid orifice much dilated and valve incompetent, one of the segments bound down to the sides of the ventricle; the condition of the valve was such as must have permitted free regurgitation during life. Walls of left ventricle thickened to about three quarters of an inch, cavity not dilated. Left auricular walls of normal thickness and cavity of normal dimensions. Mitral valve slightly thickened, but pliable and competent. Aortic valves competent. Texture of heart fatty and friable.

The ordinary signs and symptoms in this case pointed very definitely to mitral regurgitation as the predominant trouble. Especially the auscultatory phenomena declared this diagnosis, nor was any thing developed by the usual methods of examination to cast a doubt upon the correctness of this view. It was a case in which the physician, however skilful in cardiac diagnosis, confined to ordinary methods, would inevitably commit the error of assigning the regurgitation to the mitral orifice.

We will now examine the graphic records, and see what light they might have shed upon the case. Allowing for the effects of the respiration and the irregularity shown in the middle of fig. 1, the only positive peculiarity of the cardiac trace is the high auricular wave. The carotid trace is free and ample in fig. 1; shows feebleness in fig. 2. The cardio-carotid interval measures $\frac{1}{13}$ second in 1, and $\frac{1}{14}$ second in 2, with pulse-rate in both 85 to 90 per minute. These intervals are strictly normal.

If free mitral regurgitation had existed, the cardio-carotid interval would have been abnormally long; it would have measured somewhere between an eighth and a quarter of a second. This important fact, established by my own observations, still awaits the case to prove that the lesion may

exist without the sign. In the present case I thought I had found the exception to the rule, and stated to Dr. Isham that my sign had failed me, or the case was one of tricuspid instead of mitral regurgitation. I did not feel justified in arraying this simple negative sign against what appeared positive indications of mitral incompetency, and so was almost ready to concede in this instance default of the graphic indication, when the autopsy verified the integrity of the mitral and insufficiency of the tricuspid valve, attesting again the significance and trustworthiness of the graphic testimony.

CASE 2.—John W., aged 48 years, a patient in the Cincinnati hospital, suffering from heart disease, and whom I was given the opportunity to observe by the kindness of the attending physicians. He was troubled with cough and dyspnœa, and had a double murmur at the base—the diastolic predominating,—and a systolic murmur at the apex. Apex displaced downward and outward. Simultaneous traces, fig. 3,¹ of the heart and carotid and of the heart and radial were taken February 12, 1881. The

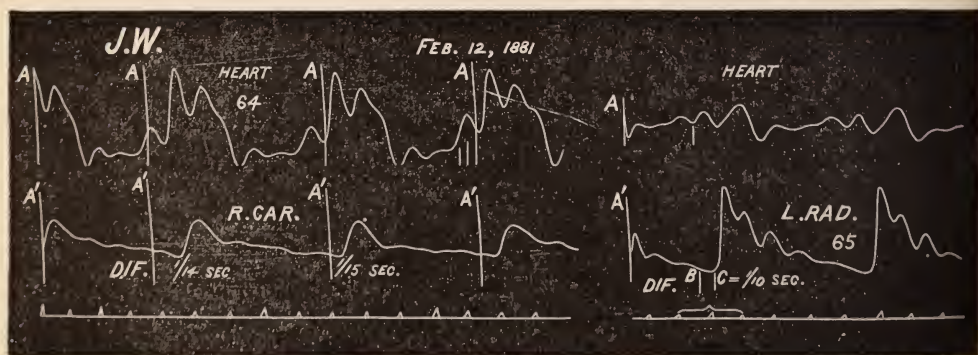


FIG. 3.

¹ Of the cardiac and carotid traces the former is excellent, while the latter is somewhat undeveloped in consequence of thickness of the tissues overlying the artery. Of the cardiac and radial traces the latter is excellent, while the former is undeveloped by a faulty application of the base. It is to be regretted that the last imperfection had not been corrected by a repetition of the experiment. However, the traces all show distinctly, and in proper position, the basal points, and so permit of correct determinations of the time-differences.

man died, and a *post mortem* was made April 19, 1881. From the hospital notes and personal inspection of the specimen, I gathered the following history: Marked dilatation of both ventricles and hypertrophy of ventricular walls. Right auricle dilated, and walls thickened. Tricuspid orifice contracted so as to admit only the tip of the little finger; valve thickened, and manifestly insufficient, as well as directly obstructive, during life. Pulmonary valves normal. Mitral valve normal. Aortic valves slightly thickened, and one contained a small calcareous plate; yet they preserved their normal form, and were evidently competent during life. Aorta dilated from near its origin to its bifurcation into the iliacs; especially the transverse and descending arch was dilated into a cylindrical pouch of considerable size; aortic walls thickened, rigid, and pervaded with calcareous plates varying from a quarter of an inch to an inch in diameter.

In this case no definite diagnosis had been arrived at; especially the tricuspid lesion had not suggested itself. The double murmur at the base was thought to indicate aortic obstructive and regurgitant lesion; and the systolic apex murmur, mitral regurgitation; cardiac hypertrophy only was distinctly recognized. A correct appreciation of the graphic showings would have demonstrated the absence of grave left-side valvular lesions. If aortic stenosis had been present the pulse-trace would have been characteristically deformed, and not free and shapely, as shown, notably in the radial. If there had been obstruction at the aortic valves, or regurgitation at the mitral valve, one or both, the succession of the arterial pulses on the heart would have signalled abnormal delay instead of the intervals shown. If free aortic insufficiency had existed, the carotid and radial pulses, besides presenting the characteristic form of this lesion, would have appeared much sooner after the systole of the ventricle than the time indicated. And further, if pure mitral contraction¹ had existed to produce

¹ Opportunity for tracing mitral contraction has not offered, but recent experiments on the schema indicate that the pulse is abnormally delayed, also, in this condition.

such distress, ventricular systole would not have shown such development, nor the arterial pulse such fulness and amplitude.

Then, as the evidences of grave valvular disease were unmistakable, and the graphic records proved the integrity of the aortic and mitral, the localization was driven to the right-side valves, and attention to the signs of the graphic method would have secured a diagnosis which could not have been arrived at by other means.

The traces sufficiently indicate hypertrophy of the left ventricle, but they demonstrate also the presence of auricular hypertrophy. The latter information is furnished only by the graphic method; when either auricle is hypertrophied there will be in the cardiac trace a correspondingly high auricular wave.

Finally, we find in the traces evidences bearing upon the arterial rigidity. The cardio-carotid interval being $\frac{1}{18}$ second, and the cardio-radial $\frac{1}{10}$ second, would give only $\frac{1}{30}$ second for the carotid-radial interval. This is less than half the average normal time-difference between these points in healthy adults, and as arterial rigidity is known to be the greatest of all factors increasing pulse-wave velocity, such unusual speed as is here signalled indicated that the arteries of the upper extremity partook of the rigidity so strikingly displayed in the aorta; and this rigidity extending in the innominate and carotid was the undoubted cause of the unduly short cardio-carotid interval. It is probable the pulse-wave velocity will come to be accepted as a test and gauge of the quality of the arterial walls as to their degree of rigidity or pliancy.

The diagnostic signs furnished by cardio-sphygmography go hand in hand with the evidences of auscultation and percussion, correcting, extending, and completing the latter, and give to cardiac diagnosis unparalleled precision and perfection.

THE PATHOLOGY AND PHILOSOPHY OF SEA-SICKNESS.

BY C. PITFIELD MITCHELL, M.R.C.S.

DURING the summer months of last year, as surgeon on the Atlantic mail service, I was afforded opportunities for studying the very interesting chain of pathological events exhibited in sea-sickness, and sharing the existing want of a rational synthesis of these phenomena, I have colligated the facts and worked out the conclusions which I propose to embody in the present paper.

An investigation of the pathology of this disease is facilitated by the absence of circumstances which usually veil the relations of cause and consequence. Unlike many kindred inquiries the conditions are simple. In the first place, the oscillating of the vessel on its transverse and longitudinal axis, as the primary initiator of the functional disturbances, stands out with unmistakable reality; then this oscillation is roughly measurable in its increments and decrements, and varies together with measurable increments and decrements in the pathological effects. In other words, the factors and their products may be known quantitatively, which is seldom possible in the investigation of morbid phenomena outside the laboratory. The effects, too, are few in any given case, and but little complicated in their mutual connections, and may also be observed in an indefinite number of instances under similar conditions.

Although my own observations will form the groundwork of the following reasoning, yet since these are not fundamentally at variance with those of others, the statements of fact may conveniently be more dogmatic than would be otherwise permissible.

Between the symptoms first felt in the well-marked, and those present alone and throughout in the mildest examples, there is a likeness in kind; in both they are subjectively cerebral. Disturbances of the intellectual centres are known by confusion of thought, mental torpor, an inability to fix the attention, and volitional weakness; changes from the usual currents of the sensations and emotions are evidenced by frontal and occipital headache, cerebral throbbings, intolerance of sensory impressions, irascibility, dejection, and despair. The physiological susceptibility existing, there will be faintness and vertigo, nausea and vomiting, and an inclination to remain recumbent and to sleep. A physical examination would then elicit the following conditions: pallor of the face, coldness and clamminess of the extremities, the pupils probably normal, the circulatory and respiratory functions depressed, the pulsations being slow, frequent (?), and small; the arterial tension low, the temperature normal or subnormal, the tongue clean or covered with a thin whitish fur, thirst, the bowels constipated, urine scanty and concentrated. Such, abstracted from my own experience, is an average of the signs to be met with in all stages of the disease. It will be necessary to dwell upon some of these signs in detail to point out some variations from the average, the frequent co-existence of other independent and obscuring symptoms, and the circumstances under which they severally arise. The subjective phenomena vary, of course, slightly in character, and widely in degree, with the individual organism and the degree of pitching and rolling. Those especially predisposed may be

quite unable to remain in the upright posture. I can recall four cases where the fainting amounted to asthenic syncope. In these cases there appeared to be a deficiency from the beginning in the reactive forces of the body, and the sickness and want of food were unusually protracted.

Generally the vomited matter consists of watery fluid, with more or less bile intermixed. Exceptionally, if the straining be excessive, slight hæmatemesis may supervene. The circumstances which influence the degree of vomiting may be mentioned incidentally, that the argument to follow may not be anticipated. In the first stage the epithelium of the tongue undergoes little change; later the organ is thinly furred, and the tip speckled. Gastric catarrh, so frequently a concomitant of sea-sickness, is probably determined by exposure to other unhealthy conditions of ship-life, chief among these being impure air and water, and stale food. That it is not a primary symptom, and may have an independent origin, can be known and inferred from the facts that it usually appears late in the attack, *i. e.*, after exposure to the above conditions, is often persistent during convalescence, and is found among those who enjoy an immunity from sea-sickness. Salivation is present in a considerable percentage of cases, but whether it properly belongs to the group of symptoms directly resulting from the ship's motions, is, I think, uncertain. The best examples I have seen were associated with gastric catarrh.

Of twenty-nine recorded cases—a fraction only of the number actually observed—constipation was present in twenty-eight, in the remaining one early diarrhœa and subsequent constipation. So constantly are the bowels confined from the commencement and during the continuance of the attack, that when they are not so it may always be confidently attributed to extraneous causes. And this, too, after duly allowing for the presence of the hydrated

peroxide of iron in the water used for drinking and cooking. The water for such purposes is stored in iron tanks, which, unless frequently cleaned out, contain a thick sediment of iron rust, very easily stirred up in rough weather. That used for drinking is sometimes filtered, but not so the water used for cooking. This, as one of the causes of costiveness at sea, especially in those who are not sea-sick, does not appear hitherto to have been noticed. The fæces may show the characteristic staining. The average temperature (buccal) drawn from the notes of twenty-five cases was $96^{\circ}.7$; the maximum being $99^{\circ}.2$ and the minimum $94^{\circ}.6$. Respecting the differences of temperature, the general conclusions may safely be made, that an elevation is either indirectly caused by the co-existing constipation or has some origin unconnected with the cause of sea-sickness, and that if the sickness be moderate and quickly rallied from, the temperature will be normal; if, on the other hand, severe and lasting, it will fall. There is no sign more distinctive than the enfeeblement—it appears to be nothing more—of the circulatory and respiratory functions. Occasionally, where vomiting is absent and some other disease is suspected, the finger on the pulse may suffice to decide the diagnosis. The average pulse-beat in twenty-eight cases was found to be 78; the highest being 116, and the lowest 56. This, as a typical average, should be accepted with reservation, for the cases recorded were all among the emigrant class of passengers, whose hygienic and social surroundings were likely to complicate the pulse-beat of sea-sickness. Also, the observations were necessarily taken indiscriminately, with the patients lying, sitting, or standing. It is important to note the invariably low tension of the pulse. The urine is diminished in quantity, of high specific gravity from a relative increase of normal solid constituents, without albumen or sugar.

The next step is to bring into view the series of changes by which the external incident force works that internal pathological state of which the symptoms set forth are the outward and visible signs. The veritable *questio vexata* of sea-sickness must now be examined, and the right answer discovered, before a complete and scientific *rationale* of the disease can be attained. A brief retrospect of past inquiry in this direction will afford light and guidance in the present search.

Sir James Alderson¹ cites an experiment made by Dr. Wollaston, 60 years ago, to the effect that "if a barometer be carried out to sea in a calm, the mercury will rest at the same height as when on shore ; but when the ship falls by the subsidence of the waves, the mercury is seen apparently to rise in the tube which contains it." Using this experiment to explain the pathogenesis of sea-sickness, he remarks that "the approximately rigid brain and vessels are carried downward, the blood remains by its own inertia, and the consequence is to crowd blood into the vessels of the brain, and so press with increased force, producing a certain shock ; this shock and the attendant pressure produce sickness and vomiting." Dr. Pollard,² pointing out the fallacy involved therein, rightly observes that "at sea the falling of the ship is due to the action of gravity. There would be no tendency, therefore, for the mercury in the barometer, or the blood in the body, to commence falling later than the tubes which contain them. They would all begin to move at the same instant. It is, nevertheless, a fact that the action of the waves does cause the mercury in the ordinary barometer to oscillate when the sea is rough. As already shown the mere falling and rising of the ship would not account for this ; it is the sudden striking of the ship by

¹ "Observations on Sea-Sickness." *British Medical Journal*, March, 1872.

² "Remarks on the Pathology and Treatment of Sea-Sickness." *British Medical Journal*, June 8, 1872.

the waves which in rough weather jerks the mercury up the tube ; and in order to reduce this to a minimum, certain ingenious modifications have been introduced in the construction of marine barometers." Dr. Pollard, therefore, rejects this explanation ; he thinks "the most probable theory of sea-sickness is that held by Dr. Carpenter, Mr. Bain, and other writers, who consider that the mental and bodily prostration and the other symptoms arise from continued action on the brain of a certain set of sensations, more particularly the sensation of want of support. The phenomena of sea-sickness appear to be due to the constant repetition of this feeling of loss of support consequent on the pitching and rolling of the ship, more particularly the former." Without diverging to show the incompatibility with the admitted facts of this hypothesis, and the cognate one of "association of sensations," it will be more profitable to proceed to find, if possible, a mechanical interpretation upon which may be constructed a synthesis of all the data we possess relating to sea-sickness.

First should be considered, in juxtaposition, the nature of the force in action and the body acted upon. The rise and fall of the vessel in pitching and rolling is seen to be a rhythmical, *molar* motion, having relatively large amplitudes of moderate uniformity. Now whatever be the degree of motion communicated to the organism as a whole, and to its parts in respect of one another, such communicated motion must partake of this character ; it must *in the first instance* also be molar. Molecular changes in the brain propagated through the organs of sight may be excluded, for sea-sickness occurs whether the eyes are opened or closed. The body being composed of fluids and solids, of parts having different mobilities, we shall look for disarrangements among these as the primary internal changes induced by movements of the whole through space. *Molecu-*

lar disarrangements, if induced at all, must be secondary. But the organs and viscera are intimately joined together by their correlated structures, and by connective tissue, and much of the blood circulates transversely to the direction of the incident force, therefore only that moving vertically will undergo any appreciable displacement.

For our present purpose we may superficially regard the vascular system as consisting of arteries and veins ascending from the heart to the head and neck, and arteries and veins descending from the heart to the lower extremities. In both cases the vessels leave the heart as tubes having large sectional areas, and terminate at the peripheries as tubes having small sectional areas. How then will the blood circulating in these vessels comport itself when the body is subjected to rhythmical elevations and depressions? As the body falls, the blood-pressure both of descending and ascending vessels will remain unchanged, for vessels and contents will gravitate together, and as the body rises, to the descending set of vessels and contents the motion will be transmitted equally throughout the system—arterial, capillary, and venous,—and the balance will not be disturbed. What now will be the effect upon the blood of the superior vessels when the ship ascends? In so far that the arterial trunks are dependent, and end above as capillaries, and the walls are rigid relatively to the contained blood, we should anticipate a recession of blood to the aorta; but from the end of one systole to the commencement of the next the semilunar valves are closed, and the supported column of blood would receive an upward acceleration, together with the heart and vessels, and during systole, the heart rising with the body will impart this motion to the blood at that moment being propelled; therefore, the tension throughout the arteries will probably not be directly influenced. Note, however, the effect upon

the venous blood-pressure. When the body rises, the descending blood will be relatively accelerated, and the pressure will subside at the periphery. That this will happen seems inevitable; the upward motion of the body and vessels cannot be at once communicated to the enclosed blood, for the veins increase in calibre toward the heart, are without valves, and are not normally distended to their full capacity. In order to understand the mechanism more fully, attention should be given to the researches of Dr. Moxon, "On the influence of the circulation upon the nervous system."

The following passage¹ will serve to make clear the point we are considering: "The way by which the cerebro-spinal fluid enters the cranium and brain is by a relatively wide and free passage; while the way by which the blood enters the cranium and brain is by the relatively narrow passages of the capillary arteries; and if the circulation be feeble, the heart may drive the blood less forcibly to the texture of the brain than the venous pressure in the spinal canal drives the cerebro-spinal fluid into its cavities, so that when the head is raised and the venous blood falls away more readily from within the skull, the cerebro-spinal fluid may outstrip the arterial blood in the race to supply the place. And thus the ventricles of the brain may tend to fill with water more quickly than the substance of it with blood, and so the brain blanches and the person feels giddy." Knowing, then, the effects upon the cerebral circulation of a single upward movement of the body, we are in a position to follow out the sequences, under certain physiological conditions, when the motion is compounded of elevations alternating with depressions. At the close of the first ascent the blood in the arterial capillaries is lagging behind that in the venous capillaries; and during the descent, by increased

¹Croonian Lectures: *British Medical Journal*, April, 1881.

cardiac action equilibrium is partly restored. With the next ascent the pressure in the venous capillaries is again lowered, but now more readily than before, for the heart, assumed to be incompetent to maintain the balance from the beginning, has been further incapacitated by the previous drain upon the cerebral vessels, and by the interference with its normal rhythm in the medulla oblongata, and probably by varying pressures in the right auricle¹ and sinus. The series of motions continuing, it is easily seen that a relative downward impetus to the venous blood and an increasingly uncompensating rise in the arterial blood must end eventually in anæmia of all parts of the brain. And this completely harmonizes with the mode of onset of sea-sickness; it is not sudden, but gradually progressive. It thus appears that the cause of sea-sickness is in the *rhythm* of the motion to which the physiological subject is exposed. If the upward force were continuously in action the initial disturbance would soon be readjusted through the heart, which in this case would not be distressed by interferences with its organic rhythms.

Taking with us the above deduction, and following out its corollaries, we may compare these side by side with the inductions.

A sufficient circulation of healthy blood in the cerebrum being one of the conditions essential to normal psychical processes, viewed only as to their quantity, if the circulation fall to an undue level the strength of the intellectual actions will also decline. As we have already seen, in sea-

¹ This inference is interestingly corroborated by Dr. Gaskell's results of his novel experiments on the action of the frog's heart, brought forward at the recent International Medical Congress, and upon which I have fallen since these lines were written. He remarked that "any influence acting upon the auricles and sinuses alone which can alter the rhythm, such as heat, cold, atropin, muscarin, etc., makes the rate of both auricles and ventricles synchronously quicker or slower, while the same influences affecting the ventricle alone produce no alteration in the rate of rhythm of either auricle or ventricle. We may conclude, then, that *the cardiac muscle in the normal heart contracts to each impulse which reaches it from the sinus or auricles*, so long as the muscular tissue is in a condition in which it is ready to contract."

sickness the thoughts are sluggish, the attention cannot be focused, the predominant wish is not to do any thing, and there is a disposition to sleep.

The accompanying emotional states also become intelligible by joining this physiological principle with a certain law of mental evolution. The nervous plexuses by which the conduct is adjusted to social requirements are among the last evolved in the race, are among the least permeable channels for the discharge of nervous energy, and, when the pressure is high, are the courses taken by the overflowing force; if, then, the pressure fall, if much of the circulatory energy—the expression is used advisedly—be drawn off with the blood, the residue will run through the older and better organized channels, the plexuses for aggression and defence. In like manner, depression—note the word—of spirits is interpreted.¹ The facts of sea-sickness in this connection may be recalled. Indications of ill-temper I have noticed so frequently in both sexes, and in those distinguished at other times for their amiability, that I have learned to look upon such manifestations as pathognomonic signs, while the dispiriting influence of sea-sickness is proverbial.

As coöperating indirectly to produce these subjective states, the molecular vibrations set up throughout the body by the action of the propelling apparatus of the vessel, and the muscular tensions caused by the latter's movements, as a whole, should not be overlooked. Among the effects of these forces which may be said to do work upon the organism while this is passive, are doubtless the indolence and large appetites of those who are in health at sea. Although paleness of the face is possible apart from cerebral anæmia, yet with the inference that all the structures above the trunk are depleted, the fact agrees. The functional strength of the heart and lungs—and not unlikely of the

¹ See Spencer's "Principles of Psychology," vol. i, pp. 603 to 605.

vaso-motor nerves,—being derived from the medulla oblongata and ganglionic systems, will wane if the blood circulating in the medulla be much diminished. This, which physiology teaches, pathology abundantly ratifies. And to the extent that vomiting is admitted to be a symptom of cerebral anæmia, this also might be taken as verifying the deduction. If, however, the relations of the pneumogastric and phrenic nerves to the large vessels in the thorax be borne in mind, we shall perhaps be led to think that these phenomena are caused ento-peripherally. The right pneumogastric nerve passes between the right subclavian artery and vein; the left, between the left carotid and subclavian arteries and behind the left innominate vein; and the phrenic nerves pass over the first part of the subclavian arteries, between them and the subclavian veins. Now upon some of these structures the incident force will be concentrated. As the blood-pressure at the venous periphery falls, the pressure at the centre will rise, the heart being unable to distribute the blood of the surcharged venous trunks; and though the facts show a corresponding fall in the arterial tension, it is at least possible that the normal relations of these nerves to these vessels may be so changed that the functions over which the nerves preside may be perverted in the manner observed. The distribution in the thorax of the little-known sympathetic plexuses only helps at present to confound our knowledge of the actual factors. The intensity of the vomiting in sea-sickness points with some cogency to an ento-peripheral genesis of this symptom; it is difficult to reconcile with an anæmic state of the brain, so large an evolution of nervous energy from the central system as the vomiting in this disease implies. Continuing to follow out the sequences, as the arterial tension throughout the body slackens, and the general circulation flags, conspicuous functional changes may be sought at the external

and internal peripheries. And there in sea-sickness they are found. The hands cold, clammy, and bluish ; the bowels deficient in natural secretions and peristaltic power ; the eliminations of the kidneys less in quantity and of greater density than normal, are clearly among the immediate consequences of an imperfect circulation. But here again the antecedents are compound. Since the performance of every organic function is aided by the general diffusion through the system of continuous centrifugal discharges of nervous energy (Spencer), a considerable subtraction from the chief source of these, the brain, will be a derivative and conjoint cause of this general functional quiescence.

The observed low temperatures are similarly explicable. There cannot be ascribed to thirst in sea-sickness the meaning we give to it in most other diseases ; general tissue-changes and aqueous loss by the lungs, skin, and kidneys being reduced, this feeling must be otherwise created. Sensations of this class probably depend upon the physical state of certain sensory end-organs of the alimentary canal which have immediate contact with alimentary material, so that if, as by constant vomiting, the fluids normally bathing the mucous membrane be exhausted, thirst may be produced irrespective of the general needs of the organism. Illustrations of this are found in the temporary relief to thirst afforded by simply moistening the mucous membrane of the mouth, and in the thirst, out of all proportion to the quantity of fluid lost, in the smoker who expectorates. In the present instance the sensation may certify to a real want, for excessive vomiting may preclude even the ingestion of such water as is needful for the most vegetative functions.

Sundry facts, purposely left for consideration until this stage, may now be presented as additional support to the foregoing argument, and at the same time receive their

own elucidation. In all cases of sea-sickness, probably without exception, a change of posture from the perpendicular to the horizontal is very shortly followed either by a total cessation of all the symptoms or some measure of amelioration. With many persons who are fairly comfortable while reclining, even a slight elevation of the head and trunk suffices to provoke a return of the malady. That relief should ensue on lying down is so obviously in harmony with the views advanced, that to mention it is enough. Still the fact remains that in some individuals partial sickness continues notwithstanding. This may appear *prima facie* to be somewhat contradictory, but by a little reflection it may be seen that when the oscillations are extreme—and it is then that the exceptions occur,—if the horizontal position be assumed the conditions favoring sea-sickness will not have been wholly removed. And when the perturbations of function are profound, according to the interpretation here given of their pathogenesis, the time occupied in recovering; when the perturbing force is no longer present, may be expected to vary with individuals. Instances of severe relapse clearly traceable to the taking of saline and other purgatives came under my notice from time to time. In my own person I suffered twice in this way, and sought the explanation in the increased cerebral anæmia and cardiac debility which must follow an influx of blood to the intestinal canal.

What are the physiological processes involved in recovery from sea-sickness? Here we are in the region of conjecture, and at present cannot hope for more than a partial solution. Taking recovery or the natural restoration of functional equilibrium as one manifestation of the law of adaptation formulated by Herbert Spencer ("Principles of Biology"), we shall be able to comprehend the dynamical processes in a general, if not in a special, sense. Plainly,

the case before us is one of *direct* adaptation, and an example of Mr. Spencer's second form of organic equilibration. By this we know that "any disturbing force, working an excess of change in some direction, is gradually diminished and finally neutralized by antagonistic forces, which thereupon work a compensating change in the opposite direction, and so, after more or less oscillation, restore the medium condition" ("First Principles"). With this general and scientific conception of the *vis medicatrix naturæ* in seasickness, a more special one comes within reach. Adequate *a posteriori* reasons will presently be adduced for believing that cardiac weakness is the essential physiological fault; the heart is unable to cope with a new force whose tendency is to overthrow the equilibrium of the cerebral circulation. There is an excess in the incident force which cannot be counterbalanced. We may learn something by inquiring what becomes of this excess? It is dissipated in working the characteristic functional changes in the special ways already described; the heart's contractions are restrained, the sensible and insensible motions of the intestines are arrested, the evolution of nervous energy in the cerebrum and the molecular motions throughout the body are reduced to a minimum, and the whole organism, in respect of its voluntary movements, is brought to rest. If this fails to tell us *how* recovery is sooner or later compassed, it at least answers the question *why*. The heart and brain are finally enabled to equilibrate themselves to the force whose rhythm has overthrown them, because the brunt of this force is shared by all the other organs of the body. If it were possible for the surplus force to remain undistributed while the organs upon which it falls are unable to poise it, recovery could not take place, the heart must stop, the equilibration would be death. That the ultimate adaptation consists in an integration of energy at the places of inci-

dence of the external force, is to be inferred from the general principles of equilibration. The strength and permanency of the adaptation will be expressed by the product of the incident force, and the time during which it is in operation. In my own case, on the first voyage, after an illness lasting three days, adaptation was complete, but not sufficiently organized to continue throughout a week's stay on shore, as proved by some sickness at the commencement of the second voyage, and again, still less severe, after a like interval on the third voyage. Given a constitutional predisposition to sea-sickness, however deeply ingrained be the acquired inurement, according to the doctrine of evolution, if the modifying force be removed, the *status quo* will gradually be approached, though never reached.

Other things being equal, sea-sickness is moderate or severe relatively to the amplitude, period, and synchronism of the ship's oscillations. The "pitching" motion and the motion of a small boat in a "choppy" sea are particularly searching.

The nervous reco-ordinations assumed to have taken place in the newly adapted organism will be an adjustment to rhythmical motions not widely deviating from a mean; hence an adaptation to the swelling waves of the Pacific Ocean may fail on the short waves of the English Channel, as sometimes happens. Since acquired modifications of structure and function are transmitted to offspring, the children of those who pass their lives at sea may inherit the adapted nervous systems of the parents.

What functional or structural peculiarity protects the infant from sea-sickness? Not unlikely the relatively unadvanced development at this age of the brain and its blood-vessels. Probably in infancy the integration of the visceral functions in the cerebrum is still incomplete.

The answer it has been found necessary to assume to the

question : Why are some people sea-sick and others not? may now be made good. Already some grounds have been given for thinking that functional differences underlie differences of susceptibility to sea-sickness. Then it may be prevised that where the functional momenta and the potential forces of the body are below par, the moving equilibrium will be relatively unstable and easily disturbed. This we shall find inductively substantiated. Boys and girls at the age of puberty, when the expenditure for daily activities is largely supplemented by that for growth and differentiation, have appeared to me to suffer especially; females are more prone than males, and we know their physiological reserve to be generally less; temporary weakness and debility seem frequently to determine an attack; and so-called "nervous" people, with weak vascular systems, rarely escape. Contrarily, when the functional momenta and the potential forces are at par, the moving equilibrium will be more stable and not easily disturbed, which ought to be exemplified in those who are *not* liable. And so it is. Allowing for qualifications, men and women at adult age, whose health and physique are high, suffer the least frequently and the least severely; and, generally, it may be said, the better the animal, the more complete the immunity. (See Dr. Beard on "Sea-Sickness and its Treatment.") From this and what has gone before, we shall be justified in concluding that sea-sickness depends on the functional stability of the heart and brain; depends on the ability of these organs to immediately resist the antagonistic force which falls upon them. This conception of sea-sickness is submitted as the basis for a scientific treatment.

A CONTRIBUTION TO THE STUDY OF ANÆMIA.

By FREDERICK P. HENRY, M. D.,

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THE value of the hæmacytometer as a means of diagnosis, depends upon the wonderful recuperative power of the blood as regards its proportion of water. But for this tendency toward the maintenance of a certain quantity of blood in the vessels, irrespective of quality, the instrument would be of little value beyond determining the proportion of white cells to red ; and where this recuperative power is interfered with, the examination, unless made by one who has had some experience in such matters, might be in the highest degree misleading. It is interfered with in choleraic conditions, that is, in states of the system in which the waste of water is greater than the supply ; and also in conditions of imperfect ingestion and absorption of water, the waste of this fluid remaining at or below the normal.

A marked example of the latter of the two conditions above referred to, was furnished by a case of stenosis of the cardiac orifice of the stomach, which came under my notice at the Episcopal Hospital, in April, 1881.

William G., a Scotchman, æt. 56, had first experienced difficulty in deglutition five months before admission. He was then an inmate of a soldiers' home in Virginia, and the symptoms first appeared immediately after excessive vomiting caused by a

debauch. His family history was good. He had never had syphilis. He was greatly emaciated, having lost fifty pounds in five months, and suffered greatly from excessive thirst. He declared that water would not pass the stricture. With much difficulty a drop of blood was squeezed from a deep needle-puncture of the finger. It was very dark in color, and undoubtedly inspissated. An enumeration gave the following result :

No. red cells per cubic mm., 5,525,000.

Proportion of white cells to red, 1 to 550.

The normal average of red cells per cubic mm. being about 5,000,000, the above figures would have indicated an excellent state of blood nutrition, had it not been evident, from the clinical history and the physical state of the blood, that its proportion of water was greatly diminished. The term oligæmia would properly describe the condition then existing.

The patient was nourished with enemata until my colleague, Dr. Forbes, succeeded in passing a bougie into the stomach, after which he was able to take liquid food by the mouth, and rapidly gained flesh and strength. He soon obtained permission to go out on a pass, but returned exceedingly drunk, and was discharged.

As I did not make another count in this case, I can only surmise that the absorption of water and improvement in the patient's general condition were coincident with the establishment of a high degree of anæmia (oligocythæmia), a state of affairs that might be appropriately styled a medical paradox !

The more accurate diagnosis of anæmia attained through the use of the hæmacytometer, has led to certain modifications of our views regarding the pathology of several affections. By the term pathology as here used, I intend to imply the relation between the clinical history of a disease and its anatomical lesions real or supposed. By the process of blood-cell counting I have in more than one instance demonstrated that well-marked neurasthenia may exist independently of anæmia. I have also been able to confirm the observation that symptoms formerly attributed to

plethora are often due to anæmia. As to the existence of plethora I am extremely sceptical. I have never seen any morbid condition whatever that could be ascribed solely to an excessive number of red cells in the blood. I recognize a condition common to hearty eaters of animal food, which has been called plethora, but attribute its symptoms, for the most part, to albuminoid indigestion, and would classify it under the head of lithæmia. This condition has been admirably described by Fothergill.

The following case illustrates what might be termed spurious plethora.

Miss D., æt. 19, came to me complaining of a daily flushing of the face, coming on after meals, most marked in the evening, not accompanied with headache, and followed by pallor and slight sense of chilliness. At other times her extremities are warm; hands usually moist. The flushing was so annoying that she abstained from going out in the evening. On one occasion, a year before I saw her, an attack came on while she was in church—an evening service,—and was followed by a severe attack of syncope. A physician was sent for who did not think it safe to take her home until midnight.

The girl, when I saw her, was the picture of health, her complexion being remarkably good, her eyes bright, and her appearance in every respect that of a well-nourished individual. In spite of a large appetite she had a constant sense of weakness. Her menses were perfectly regular. As she had lived in a malarious district and there seemed to be a certain periodicity in the attacks, I prescribed a pill of quinine and valerianate of zinc.

A week later she returned and reported only a slight improvement. I then counted the blood globules with the following result.

No. red cells per cubic mm., 3,995,000.

Proportion of white cells to red, 1 to 530.

This showed the case to be one of decided anæmia, and I prescribed wine of iron and arsenic. Three weeks later I found her wonderfully improved. The flushing of the face had ceased entirely; the sense of languor had left her, and her spirits were any thing but low. I am confident that such cases have often

been described and treated as plethora. At the same time I admit, that some physicians of large experience have learned empirically that they are benefited by hæmatinics.

A desideratum in the diagnosis of anæmia is an instrument that will determine the specific gravity of small quantities (a drop or two) of blood. In the first of the two cases reported above, there was no difficulty in deciding that the specific gravity was abnormally great, but in slighter degrees of similar conditions this could not be determined. A color test is not corrective of the unavoidable inaccuracies of an enumeration, and is alone not nearly so accurate, as I have elsewhere argued.¹

In conclusion, I would refer to an interesting observation made by myself for the first time, so far as I know, upon a property possessed by the red blood cells of permeating porous substances. I take the opportunity of introducing it here, although it has no direct bearing upon the subject of anæmia.

On Sept. 20, 1881, I performed the following experiment: After counting the blood cells in the usual manner, I threw upon a filter the same mixture from which the count was made, and then counted the cells of the filtrate. The first count gave the following result: No. red cells per cubic mm., 6,250,000; the second, that of the filtrate: No. red cells per cubic mm., 1,770,000.

The first count is about three and a half times greater than the second, and, therefore, more than one third of the red cells passed through the paper. There is little doubt that if larger quantities were dealt with, a still greater proportion of cells would pass. The whole amount of fluid thrown upon the filter in my experiment was 2,000 cubic mm. (about half a drachm), and although the utmost care was taken to place the liquid exactly in the centre of

¹ Cartwright Prize Essay, 1881.

the cone formed by the folded filter paper,¹ the immediate effect of the operation was the spreading of the fluid by imbibition on all sides upward for the space of an inch or more. This was equal to a reduction of the fluid to about one half of its bulk. If instead of half a drachm, I had been dealing with several ounces, I have no doubt that a much larger proportion of cells would have passed through. It is scarcely necessary to say that in making such an experiment the blood must first be mixed with a substance that will prevent its coagulation. I made use of the fluid which I am accustomed to employ in my blood-cell counts, a solution of sulpho-carbolate of sodium, sp. gr., 1020.

¹The paper used was the imported German filter paper.

FURTHER NOTES ON A CASE OF FILARIA SANGUINIS HOMINIS.

By CHARLES E. HACKLEY, M. D.,

NEW YORK.

FROM February to July, 1882, there was in the N. Y. Hospital, D., who is "Case 1," described by Dr. Robert Abbe in the *N. Y. Med. Journal*, Feb., 1880. The change in his case has been so slight, and the increase of accurate knowledge of the cause of his disease is so scanty, that perhaps there is little reason for reporting it again.

But at present the inclination to refer many diseases to germs, ferments, etc., in the system, may induce readers to be lenient in their criticism of our effort to maintain an interest in a case presenting such a decided organism in the blood.

The case is that of a man 37 years old, born in the West Indies, raised there, and living there, except at intervals, until within 7 years. During his life he has had several attacks of chyluria, passing from the normal amount to 100 oz. of urine in 24 hours. These attacks would pass off almost as suddenly as they came, and the patient would be moderately well. He has been subject to abscesses, and when he entered the N. Y. Hospital he was covered with boils; but where these were emptied we could discover no worms in their contents; nor does he know of any worms having ever

been discharged, or that the occurrence of his abscesses has borne any relation to the subsidence of his chyluria.

Perhaps it may not be amiss to give a synopsis of the present views of the cause of chyluria, and of some diseases supposed to have the same etiology, such as elephantiasis, lymph-scrotum, some cases of leprosy, hæmaturia, phlebitis, furuncles, and hydrocele.

In 1872, Dr. T. R. Lewis found small worms in the human blood. They were about $\frac{1}{16}$ of an inch long, and $\frac{1}{3500}$ in diameter. From one to thirty were found in a drop. This worm he named *filaria sanguinis*; it had previously been discovered in the urine.

In the stomachs of mosquitoes, which had fed on a patient with *filaria*, Dr. Manson found from ten to one hundred and twenty specimens. He asserts that the mosquito (or at least *a certain* mosquito) does not digest *human* *filariæ*, but will digest those from dogs, which come from a different worm. Dr. Manson says: "The female mosquito, after gorging itself with human blood, repairs to stagnant water for the purpose of digesting the blood, and depositing its eggs. During this period, which lasts four or five days, the *filariæ* undergo remarkable changes. Subsequently, in a more perfect state they escape into the water, and in this advanced stage they are conveyed into the human blood along with the water as drink."

Lewis says: "When the insect is caught, shortly after feeding, and the contents of its stomach examined microscopically, the hæmatozoa, if present, will be observed to manifest very active movements, which may possibly continue several hours on the slide."

These movements are like those of the common earth-worm, when the angler is trying to put it on a fish-hook; and the *filaria* looks something like an angle-worm, but under a high power it shows a delicate filament or mem-

brane projecting from each end (as represented in the accompanying wood-cuts).



FIG. 1.—From a photograph of filaria from Dr. Mackenzie's patient, taken by Prof. Wm. T. Belfield. Filaria dead and stained. The so-called caudal sheath is well shown.

The filaria having progressed somewhat from its state in man's blood till it enters with water into his stomach, about its development after that, nothing is known. But concerning the parent, Dr. Bancroft, in 1877, says: "The worm is about the thickness of a human hair, and is from three to four inches long. By two loops from the centre of its body it emits the filariæ, described by Carter, in immense numbers." Bancroft removed a worm from a lymph abscess of the arm, and four from a hydrocele. It is supposed that the mature worm is located somewhere in the lymph system, and that the filariæ found in the blood are embryos thrown off in immense numbers from this parent.

In our patient, as in many other reported cases, the filariæ could only be found in blood drawn at night; Dr. G. L. Peabody, who frequently examined him, says he has always failed to find them in blood drawn by day, or in any portion of the urine.

It is said that if the patient is kept up at night and sleeps

by day, the habits of the filariæ are also reversed. Attempts have been made to explain this intermittent appearance by saying: (1) that the parent throws off a new brood every



FIG. 2.—*Filaria* from Dr. Hackley's patient, as observed on April 26, 1882. Drawn and measured under a No. 7 Hartnack objective, magnifying 500 diameters. Actual length of worm, exclusive of filaments, about .03 mm. ($\frac{1}{75}$ inch). (The cephalic filament is drawn too long, and does not taper sufficiently.)

24 hours; or (2) that the filariæ collect in the lymphatics or some internal organ during the period they are absent from the blood. Both theories are open to question.

Some observers in the East say nothing about this daily disappearance, so perhaps it is not universal. It is said that embryos withdrawn from the blood in the evening did not disappear for 170 hours, while those taken in the morning did so in 108 hours.

If these filariæ are embryos and do not propagate, but come either in immense broods or by constant streams from a parent worm, and this parent only enters the system by drinking water containing advanced filariæ in hot countries, we must acknowledge that the parent may be a long-lived animal. Our patient has had his present attack of chyluria more or less persistently, and has not been in the West Indies for seven years.

Dr. Lewis says: "Not only may these hæmatozoa found in man live for a period of more than three years, but there is no evidence that they have any tendency to develop beyond a certain stage as long as they remain in the circulation."

Toward the end of April, our patient complained of pain in the right iliac region, and there was some indistinct swelling there. In the beginning of May came a lump, just below Poupart's ligament, in the right thigh, consisting of several enlarged lymphatic glands. This swelling was very painful, and the resident physician thought it enlarged at night and decreased by day. About this time filariæ were very scarce in the blood, many slides being examined without finding any. It was hoped that the parent worm might have lodged at this place, and that if it were removed the symptoms would cease.

May 13th, the bunch of enlarged glands were dissected out; during the operation several white filaments, not adherent to the surrounding tissue, were encountered; one of these, about the diameter of a hair, was drawn out, from the gland tissue apparently, for about three inches, and as it

could not be drawn further and was quite tough, it was cut off and at once examined under the microscope by Dr. Peabody, who considered it nerve tissue; it was proposed to examine this more carefully at leisure, but unfortunately the specimen was lost.

Although we were not sure of having removed any parent worm, during the two months of the patient's sojourn in the hospital subsequent to the operation, no more filariæ were found in his blood, although it was examined frequently and at all times of day. He said he felt better and stronger, and his urine became clearer, some days appearing quite normal, although at other times it was as milky as ever, without discoverable cause.¹

POSTSCRIPT BY THE EDITOR.—Through the kindness of Dr. Peabody, pathologist to the New York Hospital, by Dr. Hackley's permission a number of physicians were on different occasions allowed to observe the filariæ in this case.

On April 26, 1882, about midnight, Drs. Peabody, Amidon, and myself, made the following observations. In the two or three drops of blood procured by Dr. Peabody we found very few filariæ, but we were able to study them quite carefully with several good objectives, giving from one hundred to five hundred diameters. We were particularly interested in the appearance of the extremities of the parasite, from which we all saw, or thought we saw, filaments project. These were excessively delicate, tapering, and whip-like, the one attached to the tail being considerably longer than that at the cephalic extremity. We all noticed sudden movements of those whip-like filaments, which seemed to me movements of protrusion. This was more especially evident at the cephalic end of the filaria,

¹ The editor is informed at date of proof-reading (Sept. 19), that the patient is in the Presbyterian Hospital, and that his blood contains filariæ again.

where careful observation with five hundred diameters, by a good lens, revealed a sharply marked head, with an approach to a mouth or sucker-like termination, out of which the filament presented, and from within which it seemed to suddenly project and recede again.

At no time did this filament, at either end of the filaria, present flat, or, with double contour, or wrinkled, like the sheath-like projections figured in various works,¹ or as seen by Dr. Belfield in the filaria of Dr. Mackenzie's patient in the London Hospital. Some observers have seen this membrane at both extremities of the filaria; others, only at its tail.

In order that both conceptions of the nature of these appendages may be laid before the readers of the ARCHIVES, I have had two wood-cuts made. Figure 1, represents the view generally adopted, that there is a membranous sheath within which the filaria moves to and fro; this was made from a photograph of the dead and stained parasite in blood taken from Dr. Mackenzie's patient, London, last spring, by Prof. Wm. T. Belfield, of Chicago. I wish to take this opportunity to thank Dr. Belfield for his permission to make use of his photograph. The second figure represents faithfully what we saw at the New York Hospital laboratory on April 26th. The worm was carefully measured in breadth and length, and the figure is just as it appeared, magnified five hundred diameters. The delicacy and length of the filaments are also approximately exact.

¹ Perls: "Lehrbuch der Allgemeinen Pathologie," ii Theil 1879, p. 65.

"Verhandlung des Vereins für innere Medicin," Berlin, Jahrgang i, 1881-2, pp. 105-106.

Virchow's Archiv, Bd. lxxxix, Heft 2, p. 365, August, 1882.

ON A PECULIAR CUTANEOUS LESION (ULCUS
ELEVATUM) OCCURRING DURING THE
USE OF BROMIDE OF POTASSIUM.

By E. C. SEGUIN, M.D.

IN the last few years, more especially since the publication of Trousseau's lecture entitled "*Exanthèmes sudoraux*,"¹ much attention has been paid to toxic or medicamentous eruptions.

Among these, the cutaneous lesions produced by the various bromides, when taken internally, have been particularly well studied, and quite a variety of eruptions have been observed and recorded by dermatologists and neurologists.

In the last edition of Prof. Duhring's excellent work² we find a section devoted to this subject, and the following bromic eruptions are described under the head of *dermatitis medicamentosa*.

1. Acne-form pustules.
2. Brownish discoloration of the skin.
3. Simple papular eruption.
4. Confluent or molluscoid acne.
5. Maculo-papules.
6. Carbuncular acne.
7. Bullæ.
8. Rupia.

¹ *Clinique médicale de l'Hotel-Dieu*, 2me éd., 1865, i, p. 199.

² "Practical Treatise on Diseases of the Skin," 3d ed., Phila., 1882, p. 348.

Ulcers are not named in this list, and I do not know that they have ever been described; unless it be in the shape of isolated ulcerated carbuncular acne.

It has been my fortune to observe this year two cases of large, elevated ulcers upon the legs, occurring in epileptic patients using the bromide treatment, and I desire to place on record this new form of cutaneous lesion, probably medicamentous in origin.

CASE 1.—Miss C., aged 25 years, epileptic since her fourteenth year. The disease has been fairly well controlled by moderate doses of various bromides, under the supervision, in the last few years, of her physician, Dr. P. C. Barker of Morristown, N. J. Very little facial acne has appeared, and at no time has severe bromism been apparent. Some ten years ago, while the patient was in Germany with her family, there appeared some "boil-like" sores upon the lower part of each leg, which soon coalesced into ulcers, one on each leg, presenting very much the same appearance as those of later development about to be described. On the left leg there was an "ulcer," also below the head of the fibula. In a few months these ulcers healed. So far as the mother of Miss C. knows, there was no increase in the amount of bromides, or change of bromide, prior to the appearance of the eruption, nor does she remember any special treatment, other than the application of some herbs to the sores, which determined their healing. The patient, continuing the bromide (various formulæ) treatment steadily, had no further serious eruption or ulceration, indeed no special bromic symptoms, until about fifteen months ago, when large, purplish papules, like "boils," appeared upon the outer side of each leg, about four inches above the external malleolus. These discharged, leaving ulcerated spots which coalesced, forming an ulcer which increased quite steadily in size until the early spring of this year, when I saw the patient. This account of the early appearances and development of the sores is derived from the patient's mother, whose qualifications as an observer are fair. Still, the lack of professional study of the early stages of the lesion is a *hiatus* much to be regretted. It is doubtful if the ulcer was ever a common excavated one; Mrs. C. thinks that for months prior to my examination its surface was distinctly raised above the level of the healthy skin.

When I saw Miss C., in March of this year, there was a large ulcer on each leg, almost encircling it, leaving a bridge of healthy skin over the anterior surface of the tibia. The rest of the leg, at a level about four inches above the malleoli, was covered by an irregular ulcerated patch, raised from 2 mm. to 4 mm. above the skin. Its area was irregular, somewhat pear-shaped, with its largest development posteriorly. Its greatest width, vertically measured, was perhaps 8 cent. The surface of this "ulcer" was different from any thing I had ever seen. As stated above it was raised quite uniformly above the healthy skin; its edges were abrupt, almost vertical, and showed no signs of cicatricial action. The elevated floor of the ulcer was firm, grayish-red in color, with here and there an adherent crust; it secreted a fetid, sanious, puriform liquid, and bled upon being touched with moderate violence. It did not look like ordinary granulation tissue; it was much firmer, composed of larger masses, and, in fact, at several points it presented a slightly villous or rather papillomatous appearance. There was no burrowing of pus under the ulcer, and the rest of the leg was normal,—perfectly free from "boils" or papules. This description, insufficient as I feel it to be, applies to both legs; the ulcers upon them were almost precisely alike; the lesion was a symmetrical one. (On the left leg, just below the head of the fibula, was an irregularly-shaped, smooth, white and coppery cicatrix of the ulcer which appeared ten years ago, and healed in a few months.) The patient's face and neck presented hardly a trace of bromic acne, and I was told that her body was free from ulcers or other eruptions; she showed none of the other symptoms of bromism, and her epileptic attacks were still recurring in a mild form occasionally.

I was so much impressed with the firmness and elevation of these patches that I could not avoid a suspicion of epithelioma, in spite of the symmetry of the lesion which indicated its toxic or systemic origin. Prof. Henry B. Sands, to whom I then sent Miss C., decided that there was nothing malignant about the ulcers, and he looked upon them as eruptions connected with the patient's condition or with her medicines, and he sent her to Prof. William H. Draper, who also looked upon it as medicamentous eruption. Dr. Draper has recently written me as follows concerning the case:

" . . . I remember Miss C.'s case perfectly, even though I have no notes of it. . . . I examined some scrapings from the ulcer and found only granulation cells. I think it was unquestionably a bromic acne. The lesion begins, I believe in the fol-

licular structures, it is said in the sweat follicles, but I suspect in the sebaceous ones as well. . . ."

Acting upon Dr. Draper's suggestions, Dr. Barker applied pyroligneous acid to the ulcers and they at once began healing. I saw the patient a fortnight ago and found the ulcers almost entirely healed, with copperish smooth cicatrices such as we see after common ulcers of the legs.

It is interesting to note that the bromide treatment was continued unchanged.

CASE 2.—Annie L., aged twelve years was brought to me at the Manhattan Eye and Ear Hospital on February 24, 1882, to be treated for peculiar attacks which I considered epileptic, and of a form intermediate between petit-mal and psychical epilepsy. In many seizures there were hallucinations of vision. The first attacks, which appeared about eighteen months ago, after a fever, were distinctly spasmodic. In the last few months more attacks of both sorts; right internal strabismus has appeared. Much complaint of headache, principally over the right eye; grasp of right hand less strong than that of left. Further details concerning the neurosis are unnecessary for my present purpose.

The child's health seemed good; she was fairly well-colored, and her skin was everywhere normal.

She was put upon our regular bromide treatment, taking from three to four grammes a day at various times. The indications of coarse cerebral disease were so strong that I also gave her .75 grammes of iodide of potassium three times a day, and applied a few blisters behind the ears.

The epileptic attacks were at once controlled, and the child seemed to bear the medicines well. Owing to some annoyance at having to wait very long one day, the mother ceased bringing the child to the hospital; this was some time in March, and there was then no bromism or eruption.

Rather accidentally, Annie again came under my care in June, by being brought to my clinic at the College of Physicians and Surgeons. I at once recognized her, and upon inquiry found that she had had private treatment since leaving the hospital, and had taken bromide of potassium only (no iodide). The attacks had been few and slight, more of the psychic order: fear of falling down stairs, seeing animals, weeping, calling out to mother, and complete amnesia of attack. She was somewhat stupid from the bromide, but the amount given was impossible to ascertain. There was almost no facial acne; the child's color was clear and healthy.

The mother, however, stated that since leaving the hospital clinic, the child's legs had become the seat of very painful sores, which she wished me particularly to examine. Bandages were removed from both the child's legs, and my surprise may be imagined when I beheld ulcers precisely like those of Miss C., seen a few months before.

The lower part of each leg, some 5 cent. above the malleoli, was encircled by a large ulcerated patch, distinctly elevated above the surrounding healthy skin. The outline of the sores was very irregular, varying in vertical width from 3 to 8 cent.; the largest surface of each sore being on the fibular side of the leg. The edges were sharply defined and nearly vertical. The surface of the sores, raised 2 or 3 mm. above the healthy skin, was covered with brownish-black scabs and a most offensive sanies. On removing some of these scabs a rough granulating surface, easily bleeding, was revealed. I use the word granulating, but the appearance was that of a firmer, more villous, in places almost papillomatous formation, than the delicate translucent and uniform surface of ordinary granulations.

The mother, a not over-intelligent Irish woman, stated that this local trouble began in April, while using the bromide prescribed by the physician she consulted after giving up the hospital. At first the right leg was affected with large pimples or boils, which "broke," ulcerated, and coalesced into an open sore. In about a fortnight the left leg was similarly affected. It is interesting to note that this woman's account of the beginning of the ulcers agrees precisely with that given by the very intelligent mother of Miss C.

I directed that the ulcers be gradually cleaned of scabs by frequent washing, and that a strongly carbolized ointment be used twice a day. The dose of bromide of potassium was fixed at two grammes night and morning. The mother, having other children to attend to in the midst of the difficulties of tenement-house life, did very little toward cleansing the sores, and did not apply the ointment as carefully as necessary. Besides, the child dreaded to have the legs dressed, and cried violently each time they were washed. I was away a great part of the time in the months of July and August, and during that time there was no material change in the size or appearance of the ulcers. The child remained free from ordinary bromism, and her complexion was good. The seizures occurred but rarely.

On July 28th I gave her ether, and after removing nearly all

the scabs from the left ulcer, I applied to it Paquelin's cautery quite freely, and ordered a carbolized lotion for a few days, to be followed by applications of balsam of Peru ointment. At the same time I cut out a strip of the ulcer and adjacent sound skin for histological study; this was pinned on a cork and placed in bichromate of potassium solution. In cutting away this little flap I demonstrated that there was no appreciable lesion of the subcutaneous connective tissue; the derma seemed hypertrophied, but the whole piece was easily dissected away with the scalpel. I reduced the bromide one gramme night and morning, and gave five drops of Fowler's solution after each meal.

I saw the child again Aug. 21st. The ulcer which I had cauterized, that on the left leg, was nearly half healed over; the right leg presented the same appearance as at first, and in spite of my urgent orders had not been well cleaned of scabs and sanies. I directed that to be done before the child was brought again, intending to use the cautery again, or to apply nitric acid. The same amount of bromide was to be taken night and morning, and eight drops of Fowler's solution were to be given after each meal.

I have met with a third case, in which the legs were affected in a way not unlike what the mothers of Cases 1 and 2 describe as the first or carbuncular stage of the ulcer; and probably, if the bromide treatment had been persevered with, ulcers might have formed in this case also.

Miss A. B., aged 17 years, a handsome girl, with a very fresh complexion, of German parentage, consulted me on November 1, 1880, for epileptiform attacks. She was a healthy child. At 8 years had an attack of typhoid fever lasting several weeks. On getting out of bed at the end of eleven weeks, found that she could not walk. It was nearly spring (fever in August) before she walked freely. Arms were unaffected; mind normal. In her ninth year had a convulsion one morning, biting her tongue. During the day legs became quite weak, especially the right. There seemed well-marked paralysis, but it is not certain whether there was any muscular wasting. Under galvanism and strychnia she gradually regained the use of her legs, but ever since she has had convulsions at very irregular intervals—every two or three weeks, or at interval of months. In the last two years three or

four severe attacks, last one in September. Has had occasional doses of bromide, but no systematic treatment. Patient states that in night attacks she wakes dizzy, has time to call some one, hears a loud noise as of a wheel going faster and faster, until she loses consciousness; hears no voices or bell-sounds. A cousin who has witnessed seizures, says that there is a severe convulsion, in which patient's eyes are open; after attack, she is stupid, and wants to rise; talks and weeps violently. Then she falls into a heavy sleep.

Last summer, was exposed to severe solar heat and had a profuse nose-bleed. Ever since has been liable to petit-mal; a whirling dizziness, followed by "faintings" in some instances. Further questioning shows that ever since first spasm she has had a third sort of attack, consisting in the sudden appearance of "balls before the eyes," followed by temporary diplopia; no drowsiness.

In 1877 Dr. Brown-Séquard was consulted, and gave Miss B. his bromide mixture. This was faithfully employed for six months. The patient then had large sores with scabs upon her legs, mostly on right. It is difficult to ascertain whether these sores were like those described above, or discrete scabbed sores, such as I have seen in other patients and termed rupia-like. There was no facial acne, and the epileptiform attacks were suspended. The bromides were stopped and the sores quickly healed.

Examination showed some anæmia and symptoms of gastric catarrh. The case was so clearly epileptic, that in spite of the former bad effects of bromides, I persuaded the patient to try them again; giving her my solution of chloral and bromide,¹ three teaspoonfuls (twelve grammes) at bedtime, well diluted. Treatment and diet were also ordered for the gastric catarrh. In eight days a "boil" appeared on the right leg, constituting a small abscess, which ruptured spontaneously on the twelfth day, near a scar of the former eruption three years ago. Dose of bromide solution reduced to two teaspoonfuls at night. On the fifteenth day several large indurated pimples had appeared around the small abscess. On the nineteenth there was quite a crop of large, purplish papules with evident tendency to suppuration on the right anterior region. This appearance, the patient said, was identical with that observed while using the Brown-Séquard mixture three years ago, and she begged me to stop the bromide. I might add that for a week or ten days arsenic and sulphide of calcium had been

¹ Chloral hydrate, 15.; potassium bromide, 30.; aqua, 200. grammes.

given to control the eruption. There was no bromism, hardly a trace of common facial acne, and the gastric symptoms were better.

I suspended the use of ordinary bromides, and gave bromide of zinc, bromide of camphor, digitalis, valerian, etc., at different times; also, at times, renewed treatment for the dyspepsia, and tonics. The epileptiform attacks have been almost perfectly under control, and there has been no return of the eruption (which passed away in a week or ten days after the bromides were withheld).

Very probably, had I persevered in giving the solution of bromides, the papules would have all undergone suppuration and ulceration, coalesced, and formed an ulcer more or less like those observed in the other cases.

This third case, though incomplete, has this value, that it bears out what the mothers of Cases 1 and 2 claimed as to the early appearances of the ulcers, viz.: at first an acne, undergoing suppuration and ulceration; the resulting ulcers merging into one sore.

The following points in the clinical history of these ulcers are interesting:

1. Their origin in acne.
2. Their progressive and semi-malignant tendency.
3. The absence of bromism in the patient.
4. The absence or slight development of common facial acne at the same time.
5. The possibility of curing them by energetic local treatment (Case 1) without omitting the bromides.
6. The inefficiency (?) of arsenic and of calcium sulphide (Case 3).

Histology.—Sections of the piece of tissue removed from the ulcer show great increase in the thickness of the rete Malpighii, with hypertrophy of the whole skin in places. In several places villousities visible to the naked eye occur, made up of all the elements of the skin thrown up and out

into a minute mushroom-like or polypoid mass. In other places proliferation of young cells has taken place in the cutis, with atrophy and rupture of the epidermal layer, and partial escape of the newly-formed tissue, constituting a sort of abscess, opening externally. In other localities the patches of inflammation were wholly circumscribed, and sub-epithelial. The deeper layers of the skin, and to a certain extent the subjacent connective tissue, are infiltrated at certain points with young cells. The papillæ, hair-follicles, and sweat-glands do not appear to be the seat of any primary or important inflammatory change. In no part of the section was its surface (edge) covered by granulation tissue, as in a common ulcer.

From these appearances we may conclude that the ulcer resulted from a dermatitis, which was partly suppurative, but largely hypertrophic.

EDITORIAL DEPARTMENT.

POST-GRADUATE INSTRUCTION.

A number of physicians of this city, mostly specialists, of conspicuous disparity in reputation as men of science or as teachers, have formed themselves into a faculty of medicine, and propose to organize a college for the purpose of teaching certain things, mostly matters which come within the limits of specialties, to graduates in medicine. So sanguine are these gentlemen of success, and so confident of the righteousness of their cause, that at least one of them has appended his new professional title to his name, on the title-page of a book— before the college has been opened and before one lecture has been delivered.

There is something captivating in the term post-graduate instruction, or post-graduate course, and something imposing in post-graduate faculty ; and very naturally one interested in medical education is at once led to inquire whether there is any need of post-graduate instruction as such (apart from that never-ending effort at self-instruction which intelligent and conscientious physicians make through their lives), and whether the proposed new faculty is indispensable for the purpose, or whether an even better course of advanced teaching for the senior medical student could not be organized in the colleges already existing, and graduates invited to follow this part of the curriculum.

The first question, is there any need of post-graduate instruction ? can, I regret to say, be answered only in the affirmative. Taking the men who are yearly sent out of our medical colleges

with diplomas, we find that in spite of all they have acquired at the hands of their alma mater they are in need of more teaching, at least of more practical, educating teaching. A proof of this statement lies patent to all, in the fact that the very best men of these medical classes, those who have passed the most brilliant examinations and shown that they have crammed successfully, the very honor and prizemen of the schools, immediately compete for hospital appointments which will make them pupils again ; they will give themselves another and a better chance at self-improvement. This is a tacit acknowledgment of the incompleteness of their college education ; and if these well-crammed young men feel this way, and admit as much, what must be the condition of the rest of the hosts of graduates who, besides lacking practical education and training, are less well provided with memorized knowledge. They all need post-graduate instruction, and many of them will always need it and be glad to come for a few months to a large city to attempt to do what they ought to have done, or, more justly, ought to have been enabled and obliged by their alma mater to do before receiving a diploma. It is from this very numerous class of graduates of imperfect medical schools, who are men of all ages, of various degrees of practical success, but of one mind as to their need of better training, that the proposed post-graduate medical school is to be supplied with pupils or followers.

At the present time each medical college in our great cities bears upon its matriculation lists the names of many such seekers after scientific improvement,—men who need training and special teaching. They find themselves lost in a crowd of youths who are struggling to learn enough by rote, through books and lectures, to pass examinations of greater or lesser severity. The graduates find the lectures, as a rule, to be a mere rehash of what is in the books, made for the purposes of keeping up the time-honored plan of lecturing to large classes, and to give the student a sort of repetition of the various subjects. The impressions gathered through two senses, reading the matter by one's self and receiving it through the ear in an amphitheatre, are supposed to be more

firmly fixed, better registered, and more definitely associated in the brain, than they would be if derived only through reading. That there is a degree of psychological truth in this no one will deny, but the crying evil of routine didactic teaching cannot be sustained on such a plea. Besides hearing lectures which are repetitions of chapters of books which he knows well enough, and therefore useless to him, the graduate finds no lectures given by the only men about the schools who could teach him something new and vital, viz.: the specialists. They are, as a rule, not allowed to lecture in the regular course of lectures, and are placed in charge of clinics.

To these the graduate goes only to be disappointed and hindered in his work of self-improvement. He finds most clinics conducted without the least system, being merely an exhibition of cases as they happen to present themselves, with remarks by the professor. In many clinics, extraordinary cases of very little importance to the physician are made to play a great rôle, consuming time which should be devoted to the practical learning of methods of diagnosis and therapeutics, and to the systematic study of common diseases. Lastly, the graduate finds the front seats of lecture-rooms at the clinics crowded by junior students, who are there without appointed tasks, and who come to the clinics partly from curiosity to see disease, and partly because they have a dim notion that they must get all the "clinical experience" they can, even in their first years. The machinery of most of our schools is as yet so inadequate that there is no means of excluding the junior student from lectures and clinics which can do him no good, which use up time which should be devoted to a study of the fundamental branches, and where he occupies a seat which the undergraduate or graduate has a moral right to.

These difficulties dishearten the practitioner who has come to a medical centre, perhaps at much expense and inconvenience, to improve his scientific and practical knowledge.

But some say to him: Why do you not take advantage of the numerous private classes which are organized during the winter session, either for cramming purposes, or to teach special subjects

in a practical way? This our practitioner does, but he finds it quite expensive, and also has to encounter another difficulty, similar to that which he found in the schools: the classes are organized more for the teaching of undergraduates, by very elementary methods; he does learn something, but not as much as he hopes to, and not exactly what he needs. The proposed post-graduate school will say to the numerous graduates who return to New York for self-improvement: Come to us, gentlemen; we offer a curriculum prepared specially for you; you will not be elbowed by a crowd of raw students; we will give you every desired opportunity to study in a scientific and practical manner.

It remains to be seen whether the proposed new school of medicine will succeed, by such an appeal, in attracting a respectable number of pupils.

This brings us naturally to the consideration of the second proposition, whether a better course of advanced teaching than this new faculty offers, for senior students and for graduates, could not be organized in the already existing medical schools, as a part of their curriculum.

The qualifications of the members of the new-born faculty, and the discouraging circumstances they will have to contend against, are negative points in the discussion of this question, which we do not care, and do not need, to take up.

On the other hand, the advantages which the existing medical colleges have for undertaking higher teaching are very great.

Most of their professors, and nearly all of their assistants, are connected with hospital and dispensary services, and thus the colleges can command almost unlimited clinical opportunities. These means of teaching are now allowed to go to waste, chiefly because the faculties take no pains to bring these various services into a logical system. All the men connected with the school, and who have clinical services, teach as they please, several of them going over the same ground for the same unwieldy class of students. There is no attempt made to subordinate and correlate

the various clinics in the college, in hospitals, and at dispensaries, to the plan of teaching at the college, and to certain series of didactic lectures. Indeed, it seems to be nobody's business, and "go-as-you-please" appears to be the watchword among medical teachers. An enormous amount of clinical material is thus allowed to be ill-used or not used at all.

It seems to us that, taking advantage of the disinterested desire to teach others which the talented young alumni of a medical school always manifest, and of their interested, though laudable desire to acquire a nominal connection with their alma mater, much good clinical teaching might be secured without any great pecuniary outlay; only at the cost of careful planning of a scheme, and of energetic supervision of its execution.

With each chair upon a practical branch (each elementary scientific branch, chemistry, anatomy, physiology, and materia medica, should have its laboratory with competent teachers), quite a number of younger men might be associated with some such title as clinical instructor or clinical professor, according to the professional standing of the incumbent. This title, desirable in itself, should convey with it to each appointee the various obligations to use his hospital or dispensary service mainly for the advantage of the school, to teach a number of months in each year to limited classes provided for him by the secretary of the faculty of his school; to regulate his teaching in all reasonable ways (matters of scientific opinion excepted), according to the suggestions of the professor of his branch at the school, who would thus, besides delivering lectures and giving clinical instruction himself, control and direct a large and highly efficient teaching force and a vast clinical material. By such means the senior student, or under-graduate, and the graduate in search of further teaching, could be registered and distributed in classes, to their great advantage. On the other hand, the school would have this reward, among others, that between these numerous junior teachers the liveliest rivalry would arise as to excellence in teaching and faithfulness in serving the school, and thus there would grow up a strong competing class of candidates for any vacant professorship.

It may be said that these clinical teachers would demand emoluments. We doubt it, partly because it is in the nature of young men (young physicians perhaps more especially) to labor for the sake of an honorable title, of a possible promotion to an assured position, and I place it last, but not least, disinterestedly for science's sake.

This relates to teaching of the practical branches at the bedside, without the school principally.

Now, what need the schools do to improve higher teaching within the college walls, and to offer a post-graduate curriculum better than that planned by the proposed school?

The thorough discussion of such a question would involve a consideration of the means of improving teaching for all grades of students; a much too complicated theme. We must content ourselves with suggesting some simple changes in the present arrangement which would, in our opinion, fairly meet the wants of students about to graduate and of graduates seeking higher instruction.

In the first place it would be necessary to break through the present rule in most medical colleges, that only one lecture or clinic shall be offered to the class at one time. It is difficult to find polite terms in which to condemn this time-honored (American) custom, so contrary does it seem to common-sense and to economy in the administration of a school. Here is a college building costing from forty to sixty thousand dollars, with a full staff of professors and assistants, with a class of three or four hundred students, and forsooth the whole of this machinery must result in but one form of teaching at any one time, for a few hours a day. This is comparable to an attempt to irrigate a field from an ample reservoir, but the water must be made to flow through one little aperture; the area of the field and its need of water on the one hand, and the abundant supply of water on the other hand, are made subject to a meagreness of distribution which is justified only by a senile custom. No doubt this practice had its rise in connection with the now antiquated notion that medicine was to be taught didactically, and the whole class obliged to hear

the various lectures systematically. And, in the same way, the class was supposed to attend every clinic. By all this, wholesale superficial education was reduced to a system and made easy. The truth is that from three to six lectures, clinics, or private classes might be going on at the same time, during the same hour, if the building were appropriate, and if an official had the responsible duty of classifying students and distributing them in classes.

As regards buildings : Our medical schools (in New York) have, as a rule, only two huge lecture-rooms or amphitheatres, and one dissecting-room. Even with this insufficient number of rooms, the present amount of teaching might be doubled. But in some schools more room might be had by devoting to a useful purpose rooms which are occupied by professors for a few minutes each day, merely as a convenience. Out of several professor's rooms one or more small lecture-rooms might be arranged at small expense. Besides, some of the schools could well afford to hire or purchase a private house near the school, alter it somewhat, and thus obtain two or three small lecture-rooms, and two or three laboratories.

As regards the arrangement of studies and classes : A thorough system of graded study by divided classes may be desirable, but we are hardly ready for this. No one, however, can claim that the time has not arrived for a strict division of our large classes into junior and senior classes, and to these might be added a graduate class. These groupings should be formally made by registration in the books of the official in charge of the college work, and by means of tickets and door-keepers the separation should be practically enforced.

By such means the junior students would be obliged to give all their time to the lectures and demonstrations upon anatomy, physiology, chemistry, and materia medica. They would also find time for special practical laboratory work in all of these branches, in the smaller teaching rooms of the school ; and some of the more capable members of this class would find time to study also German or French, and perhaps medical physics and botany, branches which are almost necessary to a *complete* medical education.

As matters stand now, except in the case of the well-advised and seriously-minded few, junior students wander into clinics and hospitals, anxious to behold the horrors of disease, to experience the new sensations produced by the sight of a bloody operation, and, truth compels me to add, sometimes to gratify a lewd fancy by witnessing exposures of the human form. These young men have nothing to gain by going to clinics and visiting hospitals; they waste their time, of which they have only too little to do justice to the noble sciences they need to study; they take up room which the senior student or graduate should have. We are sure that many clinics are thus made to appear crowded and astonishingly successful; yet we hope and believe that the professors of such clinics would much rather teach a selected smaller class, free from the throng of curiosity and sensation seekers. The faculty of a medical school owes it to these misguided junior students, owes it to the seriously studious senior student, and more particularly to the earnest graduate who comes to the school seeking after higher instruction, to police the class, and by all the means in its power to restrict the junior class to its legitimate studies, and to keep it out of the rooms or hospitals where the practical branches are taught.

The senior or undergraduate class, studying more particularly pathology, practical medicine, surgery, obstetrics and gynecology, and therapeutics (that ill-taught though crowning science), might continue to hear didactic lectures, since it is premature to ask the partial or complete omissions of these; but the extensive clinical scheme proposed in the first part of this paper, should be employed as the chief means of education. The class could be subdivided into sections, and distributed in such a way as to allow each student an opportunity of practically studying disease in its various forms, of acquiring the arts of diagnosis and of treatment. It would be more especially for this purpose that the utility of numerous rooms for teaching would appear. Why, for example, should not, from 10 to 11 A.M., the junior class listen to a lecture, a demonstrative lecture let us hope, upon anatomy in one amphitheatre, while the senior class is hearing a lecture upon a surgical

topic, and a selected section or two are doing laboratory work of some sort? Why in the afternoon should not sections of the senior class be at two or three hospitals attending bedside teaching or operations, and other sections busy in the same way in the college itself, or doing laboratory work?

The post-graduate class might well find opportunities for higher study in these small, well-officered clinics. This teaching, while not particularly scientific, would be practical, and would be better than any thing offered them in their student days. But much more might be done to meet the wants of the graduate class without interrupting the regular work of the school, with very little, if any, expense to the faculties, and we believe it might be done much better in the existing schools by their own teachers, than outside in a new special post-graduate school by a fortuitously conglomerated faculty.

The graduate in search of more instruction wants above all to get the opinions and newest practical ideas of specialists—of specialists in *study* if not close specialists in *practice*. He also wants (perhaps *needs* it more than he *wishes* it) instruction in methods: the education of his eyes, ears, and hands to *do*, to *execute* the things of science. He needs to be taught to manipulate all instruments, and to examine the human body thoroughly. He needs to learn practical therapeutics, including the use of electricity, hydrotherapy, massage, which are arts in themselves.

The rooms which should be added to all our present medical schools could be utilized for the teaching of these practical specialties. Lectures, clinical or demonstrative ones we mean, upon these subjects might be going on at the very same time that the regular school lectures were being delivered and clinics held. Each school has affiliated with it a sufficient number of younger specialists, or quasi-specialists, who would gladly, with a nominal honorarium or even without any, give the instruction in such a post-graduate course. Every specialty could be represented by one professor and by several clinics in hospitals or dispensaries, and members of the graduate class assigned to these, in accordance with their needs and wishes, by the secretary or registrar of

the school. The classes which would attend such very special lectures, lectures embodying the freshest and most applicable science of the day, might be small, but their members would be seriously studious or even enthusiastic.

A post-graduate faculty organized in this way, from the younger men growing up around our medical schools, would do much to increase the reputation of their alma mater as a centre of learning, and would probably be stimulated to do much for medical science by more original research than we have at present in America. Though perhaps lecturing at the same hour with the professors of the principal branches in the students' department, there would be no rivalry or interference, if the students were classified and kept classified as above suggested.

An objection to this plan of subdividing the classes, and having several lectures, clinics, or demonstrations going on at once, is that some members of the senior class need to go over part of the elementary branches before presenting themselves for their final examinations; they should have an opportunity of attending the lectures upon these subjects, and in the scheme proposed they could not do this without missing some clinic or lecture upon a practical branch. The fallacy of this objection is obvious; it is made by one who believes in the superior power of didactic lectures to instruct. The prestige of didactic lectures has been already much dimmed, and we may live to see them almost banished from schools. The objection is invalid, because an intelligent member of the senior class can review his anatomy, physiology, etc., from text-books (having already studied these branches thoroughly and practically for a year or two).

Another objection, more serious, is that a well-managed division of the class and the exercise of effectual control over the course of study followed by its members would throw a considerable additional labor and responsibility upon the faculty. True, but this would hold good of any serious scheme for improving medical education. A part of the complicated badness of our present way of teaching students, has been the irresponsible ease with which a few men have done the business. A few faculty meetings

in the year, an hour's lecture three or four times a week, with one or two clinics each week (not prepared beforehand—mere off-hand conversations about cases), and a rather busy week at the time of the annual examinations,—these have constituted the labors of a professor in a medical school. One might say that it would not be very terrible to make an addition to these duties in the interest of the class, and of medical science, but the scheme does not call for this. It would, however, demand that the gentlemen composing our medical faculties, with a deeper sense of the great responsibility they assume in attempting to educate three or four hundred young men, should carefully select one of themselves, or an outsider, a younger and less busy man than any of them, to co-ordinate the scheme of instruction. To do it well would tax the whole energy and time of a man having special executive ability. He might be termed secretary or registrar, and his duties would embrace keeping classified lists of the students, divided into junior and senior classes, and subdivided into sections for assignment to the various teachers connected with the school. He would have to see the students and teachers frequently, and secure such a rotation of these various sections as to give every student a full opportunity to study all branches of medicine. The numerous clinics affiliated with the school, held in hospitals and dispensaries throughout the city, should be, not under his control in any scientific sense, but guided and co-ordinated by him, in reference to their order of working. He could also keep a record of the various examinations passed by students of the three classes, junior, undergraduate, and graduate.

Finally, graduates submitting to such a scheme of study, devoting several months to study under such favorable circumstances, would naturally expect a certificate of their attendance and attainments. The members of the special faculty might be empowered to examine gentlemen desiring such certificates, and they could be issued by the medical school or by the university with which it might be affiliated.

NEW BOOKS AND INSTRUMENTS.

Studies in Pathological Anatomy. By FRANCIS DELA-FIELD, M.D., Adjunct Professor of Pathology and Practical Medicine, College of Physicians and Surgeons. Nos. 20, 21, 22, 23, and 24. New York : Wm. Wood & Co., 1882.

The figures indicated above are endorsed upon the last fasciculus which our author has given to the public, of his studies in pathological anatomy. It is difficult to perceive the *rationale* of the system of numbering applied in this case, for the whole fasciculus is devoted exclusively to the consideration of one matter—the pathological anatomy of acute phthisis. Just why five numbers have been printed upon the paper-cover of a contribution to a single subject, does not appear upon its face, and the explanation is not suggested by any thing contained within.

Beyond this criticism, perhaps unimportant, we have nought but terms of commendation of Dr. Delafield for the careful, patient, thorough, independent, and unbiased manner in which he has made this study, and presented the result of his own examination of a most difficult subject. He has succeeded in throwing much new light upon the labyrinth of misunderstanding and misinterpretation, and deserves, therefore, much praise at the hands of his co-laborers.

The importance of this subject, not less than the earnest and valuable work of the author in its elucidation, warrants the use of more space in the ARCHIVES, than a mere passing mention. We feel justified, therefore, in presenting to your readers a somewhat lengthy abstract of the opinions of this investigator, and the observations upon which they are based.

After briefly sketching the clinical history of acute phthisis, Dr. Delafield proceeds to depict the *post-mortem* appearances of the lungs. A considerable portion of one or both lungs is found to

be consolidated. But the consolidation is not of uniform aspect ; the greater portion of it resembles ordinary red hepatization, or is of a peculiar grayish color, different from the gray hepatization of lobar pneumonia. In this diffuse hepatization are other portions of solidified lung, of different appearance. They are of a white or yellow color, much denser than the surrounding hepatization, or they may be softened at their centres ; they are of many sizes and shapes—the smallest hardly larger than an air-vesicle ; the largest may comprise a considerable part of a lobe.

Many of the smallest of these whitish masses are scattered irregularly through the lung tissue, like miliary tubercles. They really are miliary tubercles. The large masses seem to be formed of smaller ones. If the patient live long enough, many of these masses, both large and small, will be found degenerated and softened.

The varied conditions of the lung after death from acute phthisis have been interpreted in very different ways.

Some look upon the whole process as one of ordinary inflammation, believing that subsequent degeneration of the products of inflammation produces all the lesions. Others opine that all the white or yellow masses are essentially tubercles, and are surrounded by ordinary pneumonic hepatization, which they have excited by local irritation. Still others regard the whole as a genuine combination, side by side, of pneumonia and miliary tubercles.

None of these views seem to the author to account for all the lesions. For him the pathological condition is really a complex one, nothing being gained by an effort to make it appear more simple than it is.

It is necessary to examine each of the different parts of the lesion in detail, and see what their real structure is.

I. *The diffuse hepatization* generally resembles red hepatization of lobar pneumonia, but the cut surface is smoother. It may present a peculiar gray lustre, sometimes seeming almost gelatinous. It does not appear to degenerate or soften, except immediately around some of the nodules. The blood-vessels in the walls of the vesicles are readily filled artificially. The vesicular walls, or partitions, are unchanged, save that they are overlaid with new epitheloid cells ; and the vesicular cavities are filled, more or less completely, with the products of inflammation—such as pus cells, fibrillated fibrine, large epitheloid cells, minute shining granules, and a peculiar transparent, shining matter.

Some vesicles may be entirely filled with the granules, others with fibrine, others with epithelial cells, while others still contain granules and epithelium with pus cells.

These granules cannot be dissolved by acetic acid, liquor potassæ, or chloroform, but they can be stained by methyl violet or hæmatoxylin.

This diffuse hepatization seems to have no tendency to cheesy degeneration or necrosis. The inflammatory products in the vesicles may undergo fatty degeneration, but the vesicular walls remain intact and the blood-vessels pervious. Clinical experience and physical signs tell us that this diffuse hepatization is capable of resolution, and may entirely disappear. If chronic phthisis set in, however, this diffuse consolidation undergoes marked changes.

II. *The white or yellowish nodules*—even those which present the same appearance to the naked eye—are not all of the same nature. A definite conception of the relations of small bronchi to the air-vesicles is requisite to a proper understanding of their anatomy. None of the descriptions of authors concerning this, corresponds exactly with what we see in normal and diseased lungs. They all imply the existence of a set of tubes which branch, becoming smaller and more numerous until they finally terminate in several minute tubes, into which open the air-vesicles.

In truth, the bronchi enter the lobules most irregularly. Some enter at the ends nearest the roots of the lungs; others at the side of the lobules, into which they send branches at right angles.

The air-passages, or infundibula, are given off from the end of terminal bronchioles, or from the sides of small bronchi. They are really part of the vesicular system of the lung, and not of the bronchial system. Each lobule seems to consist principally of such air-passages. Neither the bronchi nor the air-vesicles proper constitute as large a part of the parenchyma of the lung as do the air-passages. The latter seem to be made up of a succession of large vesicles opening into one another, or of an irregular large canal formed of vesicles, into which other vesicles open from all sides.

The white or yellowish nodules of phthisis seem to be formed in air-vesicles, in single air-passages, or in groups of them, as well as in and around the bronchi. They may be classified as follows :

1. *Smallest nodules*, not larger than a single vesicle or air-passage, are, in the fresh lung, not easily distinguished from the diffuse hepatization which surrounds them. They consist of a single

air-vesicle or portion of an air-passage more or less filled with "tubercle tissue," or partly with this peculiar tissue and partly with epithelium, fibrine, and pus. The vesicular walls remain intact, the blood-vessels permeable. Often the surrounding air-vesicles and passages are filled with epithelial cells and fibrine. They then constitute a peculiar hepatization which may be regarded as partly tubercular, partly inflammatory.

2. In *larger nodules*, besides the appearances noted above, the walls of the vesicles are affected. They become split up and changed into tubercular tissue. As this process proceeds, the outline of the vesicles is lost, and the blood-vessels can no longer be injected. Such nodules are identical in structure with the miliary tubercles seen in acute miliary tuberculosis.

3. *Peribronchitic nodules* partly or completely surround a bronchus or an air-passage. In size and shape they vary with the bronchus and the number of surrounding vesicles involved.

When closely examined, these peribronchitic nodules do not all seem to possess the same minute structure.

a. Some are identical in formation with the ordinary nodules of broncho-pneumonia of children and adults.

b. Others consist of a bronchus filled with pus and epithelium, the walls being infiltrated with "tubercle tissue," and surrounded by a zone of air-vesicles filled with tubercles, and around this last still another zone of vesicles filled with epithelium, fibrine, and pus.

c. Other nodules are found of the same general constitution, but the vesicular walls in the peribronchitic mass cannot be distinguished, and the blood-vessels cannot be artificially filled. After the contents of the bronchus, its walls, and even some of the "tubercle tissue" undergo cheesy degeneration.

d. In still other peribronchitic nodules, the bronchus may contain inflammatory products and the walls remain unchanged, whilst around the bronchus the air-vesicles are filled with "tubercular tissue."

In all these varieties of peribronchitic nodules a large part of the consolidation is due to changes in the air-vesicles.

Two lesions affect the bronchi directly in some cases of phthisis.

1st. The walls of some large bronchi are infiltrated with "tubercle tissue," which undergoes cheesy degeneration. This may sometimes be so advanced and extensive that no structure can be recognized. The cavity of the bronchus is dilated and contains inflammatory products, also in a condition of cheesy degeneration,

and the adjacent air-vesicles are often affected. This dilatation may be sufficient to form a cavity of some size, which may further enlarge by disintegration of the cheesy bronchial wall.

2d. There may be a general dilatation of most of the bronchi in a considerable portion of a lung, without any marked change in their walls, and with only a moderate amount of inflammatory product in their cavities. This general dilatation usually affects the medium-sized and smaller bronchi. The lung tissue between them is usually consolidated, and when cut the tissue appears honey-combed.

The larger nodules of acute phthisis seem to be of three kinds :
1st. Peribronchitic nodules, such as described.

2d. Bodies resembling miliary tubercles in other structures, but much larger,—the “tubercles massifs” of French writers, which may suffer cheesy degeneration at the centre.

3d. Nodules consisting of a number of air-vesicles in a state of coagulation-necrosis.

Cheesy degeneration (Virchow) is recognized to be a transformation of the different elements, both the old and the new, of a tissue into an amorphous mass, composed of granular matter, fat and shrivelled tissue, the result of a process of death disintegration, fatty degeneration, and desiccation, whereby is formed a totally anæmic, dry, dense, and almost amorphous mass.

Coagulation-necrosis (Cohnheim) is a term having a meaning altogether different. It occurs in infarctions where the nutrient terminal artery is plugged or obstructed. Cheesy degeneration, however, may set in at the centre of an infarct and extend so as to involve the greater part of it. Portions of tissue, the seat of coagulation-necrosis, may resemble very closely areas of tissue which have undergone cheesy degeneration. It is in the study of phthisis especially that this similarity has led to confusion.

In acute phthisis, the nodules due to such a process of coagulation-necrosis are regularly present and form a very important part of the lesions.

There are two forms of coagulation-necrosis nodules. In each there is a centre of air-vesicles filled with inflammatory products in a condition of coagulation-necrosis ; and around such a centre there may be merely a zone of ordinary hepatization, or there may be a zone of “tubercle tissue.”

In these areas of coagulation-necrosis the blood-supply is cut off. The vessels are stopped. The outline of the vesicular wall is still visible, but is recognizable mainly by the elastic fibres ex-

isting therein. The vesicular cavities are filled with polygonal cells and shrivelled nuclei. Sometimes they are filled with coagulated fibrine only. The latter are not so opaque as the others, and they give to the cut surface of the infarct a mottled aspect. There is no gradual transition between the necrotic centre and the surrounding zone of hepatization.

Cheesy degeneration may begin at the centre of the necrotic portion, and extend out into the zone of hepatization or of "tubercle tissue." In such cases it is often difficult to distinguish these nodules from the peribronchitic nodules, or from the "tubercles massifs."

Through softening of these nodules, cavities of various size and shape may be formed. In this way sometimes a large portion of lung becomes riddled by small cavities having ragged walls, the number of cavities corresponding with the number of areas of coagulation-necrosis.

The only way to account for the formation of these centres of coagulation-necrosis, is by supposing an occlusion of branches of the pulmonary artery. The lesion is quite similar to that of the white infarction of the kidney or of the spleen. It has been demonstrated by different observers that the inner coat of the arteries is often altered in phthisis. The author has several times seen an obstruction or obliterating endo-arteritis of branches of the pulmonary artery in cases of acute phthisis accompanied by coagulation-necrosis. The various sizes of these necrotic areas would seem to be related to the blocking up of smaller or larger arteries.

In acute phthisis, therefore, we have lesions of peculiar character. The disease is very far from being a simple bronchopneumonia with degenerative changes, or an acute miliary tuberculosis combined with pneumonia.

The prognosis seems to depend mainly upon the number and size of the areas of coagulation-necrosis, and it seems probable that most of these are formed at the outset of the disease.

The diffuse pneumonic consolidation may undergo resolution. The miliary tubercles and peribronchitic nodules do not occupy much of the lung, and may be transformed into cheesy material or fibrous tissue. But the areas of coagulation-necrosis destroy a considerable part of the parenchyma of the lung, and have a natural tendency to soften and form cavities.

The "tubercle tissue" referred to by the author is regarded by him as a delicate basement-substance more or less perfectly reticu-

lated, provided with nuclei, and often enclosing spaces containing very large polygonal branched cells with many nuclei—giant-cells,—the branches frequently being traceable into connection with the reticulated basement-substance.

The subject-matter above treated of is, as usual, most abundantly and superbly illustrated, there being thirty most exquisite photographs and drawings reproduced, not a single one of inferior execution. At the end of the explanatory text follow an explanation of the photos, plate i—xciii inclusive, and a general index.

The present fasciculus, now passed in review, completes the first volume of this important work of Dr. Delafield, a monument of industrious and intelligent labor of which he can be justly proud. As the several fasciculi appeared from time to time, we had occasion, in previous issues of the *ARCHIVES*, to call attention to their merits and to their defects. We have before emphasized the value of the elaborate and costly illustrations, which, with a few exceptions already pointed out, were beyond criticism. They are truthful reproductions of what a well-trained eye will see under a good microscope when the same territory of pathological anatomy is traversed in the manner followed by the author. They must therefore possess an enduring value, whatever may be the final judgment upon the theories and opinions recorded in the explanatory text accompanying them.

The first volume of these valuable "Studies in Pathological Anatomy" comprises twenty-four royal octavo parts, illustrated with ninety-three full-page plates.

The first fasciculus appeared February, 1878, the last during May (?) 1882, the twenty-four parts being comprised in thirteen fasciculi.

The subjects and the order in which they are discussed are as follow: connective tissue, the pleura and its inflammations, the peritoneum and its inflammations, pneumonia, acutemiliary tuberculosis, chronic pulmonary phthisis, acute phthisis.

It is the intention of the author to continue the publication of his studies in pathological anatomy until he has described all the lesions with which he is acquainted; but the original plan of publishing in monthly parts having been found impracticable, will be abandoned. The fasciculi will be issued of varying size and at different intervals.

In the second volume Dr. Delafield proposes to complete the description of the lesions of chronic pulmonary phthisis.

This is a work on pathology which no earnest student can

afford to pass by. The great value of the first volume increasing in excellence as it does from the first fasciculus to the last, warrants the expectation that the forthcoming volume will be at least up to the high standard of the first. [E. O. S.]

Two Hard Cases. Sketches from a Physician's Portfolio. By W. W. GODDING, M.D., Washington, D. C. Boston : Houghton, Mifflin, & Co., 1882, pp. 257.

This little volume, which the publishers have made so neat that it is a credit to the art of book-making, by its obscure title, leads one to glance at its contents. The preface implies that psychological problems are to be considered, and vague hints are thrown out as to the author's intentions ; not sufficient, however, to alarm the unwary. He shrewdly preludes his second hard case by hard case No. 1, and having fascinated his unsuspecting reader with his charming style and candid manner, by the recital of a sad case, the nature of which is left to the decision of the reader, he boldly launches out, under the heading: "Trial of Guiteau : Outlines for a Psychological Study," with a review of a subject which has saddened and sickened the public so long, that rest, for a time at least, was to be hoped for. In vain, however. The ghost of this great tragedy will reappear again and again. The essay before us is one of the first of a long series that will follow. The advantage of being first in the field, in this case, however, is more than offset by having to address an audience who, wearied and disgusted by a long, fatiguing play, take little interest in a recital of it after the last act is over. Yet, as when satiated with a certain article of food we may not only tolerate but actually enjoy it if it be served in a different manner, in this case, also, we find that the author has so spiced the disagreeable matter with choice language that half its bad savor has left it, and has served it so daintily that relish displaces disgust. The simple, yet elegant manner of its presentation makes it a pleasure to follow the author in his dramatic representation of this great case. It is principally a recital of the facts relating to Guiteau's life and trial ; yet no opportunity is lost by the author to enforce his own opinions in favor of Guiteau's insanity. His respectful treatment of the opinion of others, however, and his evident desire not to distort facts, or to misrepresent the views of his opponents, is in such marked contrast to much that has been presented to the public that it entitles him to praise. He has maintained the true spirit of historical criticism, and has shown that

it is possible to hold decided opinions without distorting them by personal animosity. On the contrary, it may be considered by some that his efforts to do justice to all, make his eulogistic treatment of the principal characters of the trial appear rather high-colored. There appears occasionally a vein of humor, not out of harmony, however, with the gravity of the subject. For example, in referring to the division of labor among the medical experts retained by the government, he states : "To Dr. Fordyce Barker, the distinguished professor of obstetrics from New York, was assigned the delivery of the definitions and limitations of the disease known as insanity."

Of the witnesses called by the defence he refers in detail to two only : namely, Drs. Kiernan and Spitzka. Respecting their testimony, he says : "The defence opened with Dr. Kiernan, a young man from the West, who was formerly connected with the hospital on Ward's Island, and now a practising physician in Chicago,—also an editor of the 'Medical Review,' and a lecturer on mental diseases at one of the colleges. It might well be thought an embarrassing position in which to place a young man, comparatively unknown, to open the medical expert testimony of this celebrated case, on which the eyes of the world were looking, though I do not know as Dr. Kiernan felt it so particularly. But after establishing his qualifications to speak as an expert on insanity, Mr. Scoville contented himself by asking him a single hypothetical question. This being answered, Mr. Scoville turned him over to cross-examination. It was apparently a pleasure to Mr. Davidge to converse with this young man. At the second question, he drew out the fact that the witness did not believe in a future state of rewards and punishments ; and though this was not, strictly speaking, a scientific refutation of the prisoner's insanity, it was, in effect, to make a Philistine of this witness to the jury, who had stated under oath that they believed in the doctrines of the Christian religion. Dr. Kiernan bore up manfully under the keen questioning of the veteran lawyer, until he drew out the further statement that five out of every twenty-five persons that one would casually meet on the street were on the road to the insane asylum, which finished this witness with the jury, for, as Mr. Davidge said, it would land two of their number there if it were true."

"The testimony of Dr. Spitzka was well delivered, and highly entertaining to the audience ; I think that really prevented its producing any lasting effect on the minds of the jury.

The doctor was in court after the issue of a compulsory process, which the counsel for the government evidently believed was done to heighten the effect of his very positive testimony for the defence. He had already criticized the district attorney's conduct of the case in unsparing terms in the public journals ; he held in perfect contempt the line of experts who sat in front of the witness-box, a feeling that was apparently reciprocated. The doctor stated that he found Guiteau insane—"the marked feature of his insanity a tendency to delusion or insane opinion, and to the creation of morbid and fantastical projects,—a marked imbecility of judgment ; and while I had no other evidence than the expression of his face, I should have no doubt he was also a moral imbecile, or rather a moral monstrosity." He had decided ideas about the 'monstrosity' that he was analyzing ; he had his knowledge well in hand, was quick at repartee, had an assurance that was all-sufficient ; he had met most of the 'bad four' on previous occasions, and was not afraid of them, and rattled off his testimony with a volubility that was bewildering, and a positiveness of assertion that was startling to science. It was soon clear that the cross-examination was going not so much for Guiteau as Spitzka, and the result was a brilliant gladiatorial contest of some hours, without discomfiture to the witness, who left the stand with the general impression in the room that this was an 'agnostic' who did know, and what he knew he knew for certain. His estimate of Guiteau was in many respects correct. 'The congenital' character of his case, in part, is at least probable ; there is some asymmetry of the head, but that of the mind is obvious ; and I have never had an hour's conversation with him without being impressed with the talk as that of an insane man, though less by its 'farrago' than its leading ideas and general tone.' But the effect of the solid points in the testimony was lost in the brilliancy of the rejoinders, and I doubt if the jury got beyond thinking the doctor a very clever man, who was fully a match for Mr. Davidge."

When the testimony and opinions of Dr. Gray are considered the author enters into discussion, and expresses his opinions more freely than elsewhere. Referring to Dr. Gray as "the medical advocate and expert for the government," who "was called upon to decide whether Guiteau should be brought to trial," he quotes his definitions and opinions at length, taking issue with him on his views of insanity. He says : "I may agree with Dr. Gray in what he says so well about 'moral insanity,' and yet be

willing to admit that those cases, of which the books record so many, had a real existence, while holding for myself that the disordered mind does not cease to be a unit, although the observed manifestations of its insanity may seem to be confined, in some cases, to the emotions; in others, to the affections; and in still others, to the intellectual powers. We cannot deny that the old masters were as keen-sighted observers as ourselves. I dislike to hear drunkenness called dipsomania, as I so often do; but I do not therefore say that dipsomania is only drunkenness. It might improve my standing with the legal fraternity if I should pronounce kleptomania only another name for stealing; but my personal observation convinces me that the insane have sometimes a disposition to steal, which is a direct result of their disease, and for which they are no more accountable than the puerperal maniac is for her oaths." "Heterodox I know it is, but observed facts compel me to be heterodox with Prichard and Esquirol and Ray, with Morel and Griesinger and Maudsley, and I know not how many others, in recognizing in some cases a condition inherent, born in the individual, and not a result of education,—a condition which writers have recognized as hereditary mental disorder, insane diathesis, insane temperament. But the difficulty is not in the nomenclature, but in getting scientific observers, our leaders in psychiatry, to recognize the fact as it exists." * * *

"It is Gray now, but it was Ray then; and I wondered, if that mental giant could come back from the shore where he has so lately gone to sleep, if we should not hear some such vigorous English as this: you cannot get rid of a fact by denying its existence. That is the difficulty, as I conceive, with Dr. Gray's insanity; he simplifies our psychological studies wonderfully; but what are we to do with those 'minds diseased' which his classification leaves out to shift for themselves? We must still keep the bounds of insanity essentially where they were, or if we narrow them, with Dr. Gray, we must set up another kingdom of diseases and call it unsoundness of mind." The author says elsewhere, and with considerable truth, that "most of the mistakes and misunderstandings in the discussions of insanity result from an artificial nomenclature of mental diseases." He adds, however, that "it is enough to have satisfactorily established the unsoundness of a mind without being curious to label it." We think it would come nearer the truth to say that not enough effort is made to classify. A true classification—the segregation of the like—is not mere labelling. The separation of the forms of

insanity into natural groups, and a recognition of the relations of these groups to each other, is required for a true classification, and this can never be accomplished as long as we are satisfied with having established "unsoundness of mind" alone.

[W. R. B.]

Epilepsy and other Chronic Convulsive Diseases: their Causes and Treatment. By W. R. GOWERS, M.D., F.R.C.P., London: J. and A. Churchill, 1881, pp. 309.

By the critical study of quite a large number of cases (1450), Dr. Gowers has given us a valuable contribution to the general knowledge of epilepsy, but it must be confessed he has been able to add nothing to our knowledge that is really new, or that constitutes an advance. The etiology and semeiology of the disease are exhaustively studied statistically and analytically; the subject of auræ being particularly well done, and the important relation of distinct auræ as indications of the seat of the morbid process (if not lesion) in a given case, is pointed out. In one half of the author's cases there was an aura, either general or limited. In giving nearly ten pages to the consideration of the "unilateral commencement" of convulsions, Dr. Gowers places himself in contradiction to his previously expressed limitation of epilepsy, for in the light of recent autopsies a large proportion of cases in which the fits begin in a unilateral manner are cases of localized coarse cerebral disease (often tumor) of the cerebral cortex. Chapter iii contains an admirable description of the spasms and general march of symptoms in an attack of epilepsy. It seems to us, however, that the author's statement that clonic convulsions (in grand-mal) are epileptic and not asphyxial is by no means substantiated. The occurrence of an asphyxial state after even a short, sometimes unrecognized (by non-experts) tonic spasm is quite certain; and the resemblance of the phenomena of this stage to those observed in asphyxiated or suddenly anæmiated animals is wonderful. In Chapter iv, minor attacks are studied in some detail. We notice the omission from the long list of forms of petit-mal, of sudden and momentary falling to the ground with unconsciousness, a variety we have seen. Momentary and very sudden dropping of the head upon the breast we have also observed. And in our experience from a close questioning of the witnesses of attacks, it would seem that momentary general tonic spasm was more common than in Dr. Gowers' record. A fixed or statue-like state of the muscles we believe accompanies many, if not most, of the varieties of petit-mal.

In connection with minor attacks, Dr. Gowers rather too briefly treats of the mental state of epileptics, epileptic mania, interparoxysmal mental state, and chronic mental failure. In giving a statistical account of this last form of psychic disorder a most important point is omitted, viz.: the relation between epileptic dementia and the new treatment by bromides. Relatives of patients, sometimes patients themselves, raise this question, and ask if the bromide treatment will not hasten or even produce chronic failure. It would be a useful investigation to determine by means of large figures, whether epileptic dementia is more frequent than it was previous to thirty years ago.

An elaborate study of "hysteroid attacks" in patients of both sexes, leads the author to the conclusion that "the two forms of disease are not separated by any fixed and impassable symptomatic boundary." Admitting this to be true, though with all due regard for Dr. Gowers' skill as an observer, we doubt the frequency of such resemblance, we still have positive diagnostic means in a consideration of the patient's general medical history, and in trial of treatment. It has repeatedly occurred to us to reach a diagnosis by the therapeutic test in instances where the symptoms of seizures were equally epileptic and hysterical. The positive test of subsidence of attacks under the influence of bromides is open to the fallacy pointed out by Dr. Gowers on p. 180, that these drugs are often powerless against petit-mal. The negative test, however, *i. e.*, the *aggravation* of hysterical and hysteroid attacks by bromide treatment, is of much greater value.

Chapter x, on the pathology of epilepsy, is unsatisfactory, as it could hardly help being. The vague and unpathological doctrine of "discharges" is ingeniously brought forward and supported. It seems to us that the author hardly emphasizes enough, as a preliminary to this discussion, the distinction which should be rigidly maintained between the theory of the pathology of the disease and that of the mechanism of attacks. In considering the former, the insufficiency of recent microscopical observations upon the brains of epileptics is pointed out. No reference is made to Brown-Séquard's remarkable labors in the artificial production of epilepsy, though this is partly justified by the quotation as a footnote on p. 209 of Dr. J. Russell Reynolds' opinion, that artificial convulsions in rabbits and guinea-pigs are not epileptic. Now, Prof. Reynolds' excellent work on epilepsy (from which this footnote is taken) was published in 1861, before Brown-Séquard had fully developed his results in guinea-pigs. That these animals,

after injuries to the cord, brain, and sciatic nerves, have truly epileptic attacks, and that some of them transmit epilepsy to their offspring, can hardly be doubted by any one who has worked with Brown-Séquard. And it occurs to us that these experimental epilepsies throw much light on the probable pathology of human cases. And in this connection we might add that a thorough discussion of reflex or peripherally produced epilepsy (in man) is a desideratum not filled by Dr. Gowers' chapter. With reference to the mechanism of the attacks themselves, it seems to us, admitting the want of sufficiency of any current theory, that the vaso-motor theory is still worthy of more consideration than Dr. Gowers accords to it; especially if it be borne in mind that the general vaso-motor centre lies very near the "convulsive centre" of Nothnagel. The importance of dilatation of the pupil at the moment of attacks both of grand-mal and petit-mal, as corroborative of the theory of vaso-motor spasm, is overlooked. No combination of "discharges" from various parts of the brain would account for this symptom; indeed, motor "discharges," as through the muscular nerves of the iris, would of necessity produce contraction of the pupil; whereas we know from the study of section and stimulation of the cervical sympathetic nerve, that the movements of the iris are largely vaso-motor.

Chapter xiii, on treatment, is not as wholly satisfactory. The details of the bromide treatment are stated more fully than in any other work upon epilepsy, but not as minutely and circumstantially as the importance of the subject and the reputation of the author would demand. The various bromide eruptions are not mentioned; only the bromide "rash" or acne; it would appear as if Dr. Gowers had not met with the other eruptions (fully enumerated in Duhring's "Diseases of the Skin," Phila., 1882) which often resist arsenic. Bromic mania is not mentioned. The most important fault in this chapter, and one which will doubtless lead many practitioners to disastrous failure in the treatment of epilepsy, is the limit of time during which bromides should be continued after a last attack. "Hence treatment should be continued, without any reduction in dose, for six or twelve months after the last fit" . . . "A good plan, for instance, is as follows: If the attacks cease on a scruple of bromide three times a day, this should be continued for twelve months; given twice a day, for three months more; then half a drachm once a day substituted, and reduced three months later to a scruple, and three months later to ten grains, which may be omitted at the end of the second year."

We earnestly believe the above limitations to be fallacious and dangerous. From a tolerably large experience, and with the support of several high authorities (Voisin, Brown-Séquard, and others) we would assert that there is no safety from a return of the disease under four years. Voisin is disposed ("De l'emploi du bromure de potassium dans les maladies nerveuses," Paris, 1875, p. 65) to place the limit of treatment (with diminished doses) at ten years. For our own part, we have notes of a number of cases of epilepsy in which the patients have been over three years without attacks of any sort, yet we are not willing to publish them as "cures." In view of these statements Dr. Gowers' (and other writers') tables of results, and figures of "cures," need considerable pruning, or must even be altogether rejected.

Considered as a whole, the work of Dr. Gowers is a valuable addition to the library of the general practitioner and specialist. Many of the faults we have been obliged to point out are inseparable from the obscurity of the subject, and will doubtless exist in the monographs on this subject which will appear hereafter.

[E. C. S.]

Traité des Névroses. Par A. AXENFELD, professeur de pathologie interne à la faculté de médecine de Paris, etc., etc. Deuxième édition augmentée de 700 pages, par HENRI HUCHARD, lauréat de l'académie, etc., etc. Paris, 1882.

Against this work, first published in 1863 with 520 pages, and now, almost twenty years later, re-issued with 1,193 pages, hardly any complaint can be entered, except on account of its bulk.

Terseness of style and condensation at the expense of the elegant language which now characterizes the book would be, perhaps, after all, regretted. The additions by M. Huchard, enclosed in brackets, do credit to the author.

A well-selected and voluminous bibliography precedes each article. As an exhaustive treatise on neuroses, it necessarily includes many diseases considered by some to be organic.

Full recognition is given to the pathologists, and free discussion to the theories of both sides. Particularly is this the case in the articles on chorea and paralysis agitans. In other disorders, as angina pectoris and epilepsy, some cases of which have an unquestionable pathological anatomy, while in others, after the most careful search, we must be content with a theoretical pathological physiology, both sides are separately and fully discussed.

The three books comprising the work are: first, neuroses of

sensibility (including neuralgias, migraine, vertigo, spinal irritation, angina pectoris, arthralgia, and anæsthesia); second, neuroses of motility (including convulsive facial tic, contractures, writer's cramp, dysphagia, spasm of the glottis, nervous palpitation, paralyses of reflex origin and following acute disease, and the various tremors); the third book treats of complex neuroses (including chorea, epilepsy, neurasthenia, catalepsy, and hysteria).

In the treatment of trigeminal neuralgia crystallized aconitia is advised in from .00025 to .002 and .003 doses, an amount to which we seldom, if ever, push the drug without getting profound and sometimes alarming physiological effects. Huchard advises a preliminary trial of .01-.04 doses of the alcoholic extract of aconite root. Here and elsewhere the treatment is not gone into with sufficient detail.

The surgical procedures for the permanent relief of intractable cases of trifacial neuralgia are not, it would seem, quite accurately enough described. Again, in two pages on the treatment of migraine, sixteen remedies are mentioned, but specific directions as to use are given in one case only. The Indian-hemp treatment of this disorder is simply hinted at.

The differential diagnosis between pains due to disease of the sciatic nerve and spinal cord is not explicit enough. He distinguishes pains from a diseased cord: "1st, by their seat in both inferior extrémities at once, in their continuity, and in certain particular points, as the soles of the feet, where neuralgic pains rarely abide; 2d, by their form, there being dull pains, formications, numbness, and pricking rather than shooting pains; 3d, by their intensity being less than sciatic pains," etc., etc.

Both authors seemingly overlooked the pains of locomotor ataxia, which may be (1) unilateral, are generally (2) shooting, and which are usually more severe (3) than sciatic pains.

In the treatment of sciatica, pages are allotted to hydrotherapy, electricity, and parenchymatous injections, while hardly a word is devoted to the actual cautery upon which we rely so much and with reason.

Under the head of chorea, considerable prominence is given to the treatment by bromide of potassium, on which the author evidently placed much reliance; while hardly the attention which it merits is given to the arsenic treatment, so long established and proven. The administration of Fowler's solution of arsenic hypodermically is advised, but no mention is made of the dilution and filtration of the solution needed to lessen its irritant qualities.

In the treatment of epilepsy bromide of potassium is advised

up to 6.-8. daily, or to the verge of mild bromism. A decided preference is given to the potassium salt.

With the exception of trivial shortcomings, some of which have been noticed, and which may even be simply differences of opinion, the book is classical, unique in its field, and worthy careful study.

[R. W. A.]

A Treatise on the Science and Practice of Medicine, or the Pathology and Therapeutics of Internal Diseases. By ALONZO B. PALMER, M.D., LL.D., Professor of Pathology and Practice of Medicine, and of Clinical Medicine, in the University of Michigan; Physician to the State University Hospital, etc. Vol. ii. New York: G. P. Putnam's Sons, 27 and 29 West 23d Street, 1882.

The second volume of this work maintains throughout the eminently practical character, which was evident in the pages of the volume which we have already referred to. Practical suggestions in treatment are multiplied, until we sometimes are forced to think that their number may possibly serve to bewilder the student. The style is easy and flowing, and that the book will maintain the popularity of the author can readily be predicted.

After brief introductory remarks on physical diagnosis, under the diseases of the larynx, the author, in discussing croup and diphtheria, adopts the view of their duality. Pneumonia receives considerable attention; the various kinds spoken of by Delafield are referred to, and, ranking with the majority, pneumonic fever, or pneumonia arising from infection, is spoken of as distinct from the ordinary form, which follows the classical course and results from exposure to cold or wet.

The various theories held as to the nature of phthisis are well presented to the reader.

Diseases of the heart are briefly considered.

The chapter on kidney lesions and their symptoms is quite clearly put forth, and a division, partly English and partly German, adopted.

The author devotes more than one third of the second volume to the diseases of the brain and spinal cord, and with some twenty pages on the subject of parasites terminates his work.

To the students who have listened to his lectures during his many years of usefulness to the institution which he serves, these volumes will serve as instructive memoirs of their former teacher, and to those who are to listen, as an aid in more rapidly acquiring his views.

[H. N. H.]

Nitro-glycerine as a Remedy in Angina Pectoris. By WILLIAM MURRELL, M.D., M.R.C.P., etc. Detroit, 1882.

As the preface says : " The object of this work is to give directions for the administration of nitro-glycerine as a remedy for angina pectoris, the principal points being illustrated by reference to cases that have been under my care. Some of these cases were published in the *Lancet*, 1879."

After a *résumé* of the mode of discovery and manufacture of the chemical, its physiological and toxic effects on frogs and cats are given. Very little time or scientific accuracy seems to have been given to this study however. The physiological effects on man are allotted twenty pages, and are pretty satisfactory.

In from two to three minutes after the ingestion of from 0.0006 to 0.003 of the drug a fulness in the head commences, which may become intense. An extreme throbbing in the head which may become general. A rushing sound in the ears has been noticed. Mental confusion going on to insensibility has occurred, and a disposition to remain quiet and to sleep is generally present. Nausea, pallor, and perspiration, some experience ; and a headache always follows, of a few minutes' to several hours' duration. In a case of epispadias, Dr. Murrell demonstrated immediate polyuria. The pulse is quickened, and, as the sphygmographic tracings introduced show, the arterial tension falls about the second minute, reaches its minimum in nine minutes, and does not entirely reappear for twenty-six minutes.

The cases introduced, twelve in number, are not all true angina, but similar. Almost all were relieved, some permanently, by the remedy.

At the close of the book the author advises the use of a one-per-cent. alcoholic solution, in doses of from 0.03 up, according to effect, every three hours, and an extra dose at the onset of the attack. Preparations in pills or chocolate tablets may be had.

[R. W. A.]

Index-Catalogue of the Library of the Surgeon-General's Office, United States Army. Vol. iii. Washington, 1882, pp. 1,020.

Americans may well be proud of the great work which is being prosecuted by the surgeon-general's office. The completion of this index-catalogue will prove a service to the medical profession throughout the entire world, and will no doubt serve to stimulate study and research. A glimpse through this third volume will

serve to show the vastness of our professional literature, encouraging some students, but probably discouraging many more. Who is not, for example, appalled at the bibliography of diphtheria, occupying 29 folio pages, in double column, mostly printed in small type? The amount of clerical labor expended upon this work must be enormous, and its fidelity extraordinary, as witnessed by the rarity of errors in the spelling of various names and titles in different languages.

The present volume includes the words from "cholecyanin" to "Dzondi," distributed in 1,020 pages. The length of the article on diphtheria has been mentioned; that on Asiatic cholera occupies 157 pages—a space out of all proportion to the importance of the disease in the practice of American physicians. Chorea takes up 12 pages; climate, 6 pages; color-blindness, 4 pages; conjunctivitis, 19 pages; convulsions, 10 pages; cornea, 16 pages; croup, 23 pages; dysentery, 20 pages.

An interesting part of the volume, and one quite unique in medical catalogues, is the large list of portraits of medical men, 4,335 in number, arranged alphabetically in 21 pages. [J. W. D.]

A System of Surgery, Theoretical and Practical, in Treatises by Various Authors. Edited by T. HOLMES, A.M., etc. The American edition, from the second English edition, revised and enlarged by JOHN H. PACKARD, A.M., M.D., assisted by a large corps of eminent American surgeons, in 3 volumes 8vo, pp. 1007, pp. 1063, and pp. 1059. Philadelphia: Henry C. Lea's Son, 1881-1882.

This standard work is issued as a companion to the three-volume American revised edition of "Reynolds' System of Medicine," published by the same firm in the years 1879-80. The two publications, uniform in style, constitute fair compends of the two departments of medicine, surgery, and medicine more strictly speaking.

The system of Reynolds was revised by one man, and suffered in consequence, because, in spite of assertions to the contrary, one mind cannot be master of our recent additions to knowledge in all the departments of internal medicine.

In the surgical treatise the names of the collaborators is a pledge that in most instances the best special knowledge has been used in the revision. Without being invidious we may mention: Tumors and cancer, by Morris Longstreth; wounds and diseases of vessels, and aneurism, by Lewis A. Stimson; fractures, by the

editor, Dr. Packard ; hysteria, by J. S. Jewell ; injuries of the head, by John A. Lidell ; diseases of the urinary organs, by Edward L. Keyes ; diseases of the nose and of the larynx, by J. Solis Cohen ; diseases of bones, joints, and muscles, by Thomas M. Markoe ; diseases of the spine, of joints, and of orthopedic surgery, by E. H. Bradford ; diseases of nerves, by Roberts Bartholow ; gunshot wounds, by Hunter McGuire ; plastic surgery, by T. G. Morton ; parasites, by Joseph Leidy ; surgical diagnosis, by the editor ; on hospitals, by Norton Folsom. An entirely new article, of marked excellence, on diseases of the skin, by Dr. Arthur Van Harlingen, is a welcome addition. If the high reputation of authors and revisers is an evidence of the excellence of a work, then this convenient edition of "Holmes' Surgery" is all that can be wished. On the other hand, the objection may be made, that adding a few notes to articles which are somewhat out of date is a little like "putting new wine into old bottles." The reader must absorb the original and then correct his knowledge by the foot-notes and inserted paragraphs ; an unfortunate process, psychologically speaking. It is probable that because of this objection many will prefer to purchase a wholly new work on surgery, even though it be the production of less celebrated pens.

[J. W. D.]

A Treatise on the Physiological and Therapeutic Action of the Sulphate of Quinine. By OTIS FREDERICK MANSON, M.D., Professor of Physiology and Pathology in the Medical College of Virginia. Phila., 1882, 164 pp.

Historical remarks occupy the first eight pages. The next eleven pages are given up to the narration of some of the physiological effects of quinine on animals. Experiments, chiefly by M^{étier} and Briquet, are quoted, which seem to indicate that quinine is first an excitant and then a sedative to the heart and brain, and that in the blood the fibrine was increased and the globules were relatively diminished by large doses.

The effects on man in health, occupying the next seventeen pages, embody the ideas of Magendie, Bailly, Giacomini, Guersaut, Piorry, Harrison,¹ and Wood ; nothing new or original, however, appearing.

A conclusion of the study is that quinine first excites the brain and heart by exciting the stomach ; that in large, repeated doses it has a calming and sedative action on the circulation and nervous

¹ *New Orleans Medical and Surgical Journal*, ii, 331.

system which, be the doses sufficiently large, finally paralyzes ; that in large doses it produces sleep and relieves pain ; and, lastly, that even in health it affects the calorific function, markedly.

As a result of forty years' observation of malarial diseases, the author advises the administration of quinine in such diseases during the hours when the febrile phenomena are naturally subsiding, which generally are during the later hours of the night. While not prohibiting the administration during the exacerbation, he says it is a very unfavorable time, on account of the difficult absorption of the salt. The stomach is very irritable, and cinchonism is more readily produced. The description of a paroxysm of remittent fever, pp. 52-58, is concise and graphic.

In yellow fever, p. 117, he considers quinine ineffectual. He thinks given early in typhoid fever it may lessen the violence of the febrile symptoms and shorten the fever.

In typhus, it is his conclusion, from the authorities referred to, that quinine does good. In the treatment of traumatic and idiopathic erysipelas it has given the author "invariable success." Almost equal faith in its power over scarlatina and pneumonia is revealed in the next few pages.

Its use in croup, delirium tremens, acute and chronic rheumatism, cholera infantum, cerebro-spinal meningitis, and surgical shock completes the therapeutical part of the book.

Notes on the "modus operandi" of the drug and the mode of administration follow. Under the latter head the oral, rectal, endemic, and hypodermic methods are enumerated. A puff for Powers and Weightman closes the book.

The great efficacy of quinine in the hands of our author is no doubt due to the engrafting of a malarial element on diverse maladies. Whether or not the presence of some palludial miasm in the author's encephalon causes the frequent repetition of the announcement of "forty years' experience with malarial fevers," we do not know. For a person who has lived so long in a malarial region, where quinine is given in all diseases, from corns to baldness, we are happy to see the author does not consider it a panacea. The fearless way in which the author handles the drug deserves all praise in these days of triturations and pellets.

[R. W. A.]

ORIGINAL OBSERVATIONS.

A CASE ILLUSTRATING THE COINCIDENCE OF DISEASES : CERVICO-BRACHIAL NEURALGIA AND ANEURISM OF THE INNOMINATE ARTERY.

By E. C. SEGUIN, M.D.

The influence of diagnosis upon therapeutics and upon prognosis has seldom been more strikingly shown in my experience than by the following case :

Mr. S. S., aged fifty-two years, consulted me June 5, 1882, for a severe neuralgic ailment of the right side of the head, neck, and arm. He related the following history : Early in the summer of 1881, he had been thrown out of a carriage upon the sidewalk, but received no evident injury. After this fall he was restless and nervous, felt badly, had more or less gastric disorder. Spent the month of August in Saratoga, but was unrelieved. About that time he first noticed pain near the right olecranon process. This pain was quite localized at first, but soon later it extended toward the shoulder ; very gradually increased in extent and severity, occurring in more frequent paroxysms. Late in the autumn the shoulder region was involved ; and in December pain was felt in the head, a little to the right of the vertex, and later behind the right ear. Downward the pain has extended to the hand ; the fingers have never been painful or numb. The pain has been somewhat nocturnal, but never periodic. The patient has suffered extreme agony for months, pain extending from the right parietal region down the neck to the right shoulder and arm. No treatment until December, then for two months Mr. S. was under the care of a specialist for diseases of the nervous system. Has recently been at the Hot Springs of Arkansas, where, with some internal

treatment and applications of hot water in bags, he was somewhat relieved. Two weeks ago was subjected to strong electrical applications which greatly aggravated his neuralgia, particularly increasing the occipito-parietal pain. Since the electrical applications, the right arm has felt big and tight (not exactly numb). After paroxysms of pain the veins of the right arm appear full. A paroxysm occurs in my office, with chief acute pain behind right ear; it is evident that the patient suffers extremely; perspires during the attack. Exertion, use of arms, or walking, causes increase of pain, or even produces a paroxysm. Fortunately no morphine habit has become established. No syphilis.

Examination: Patient presents the usual facies of prolonged suffering; is pale and thin. Seat of pain as above stated; occipito-parietal, cervical, and brachial. Right pupil a trifle larger than the left. Nerve-trunks not tender, but painful regions are hyperæsthetic in paroxysms of pain. There is no paralysis, anæsthesia, or muscular atrophy; the movements of the arm are free, except at the shoulder joint where some resistance and crepitation from false ankylosis.

The right radial pulse is very feeble, much smaller than that on the left side. The right carotid pulse is likewise much smaller than the left. The right hand is slightly swollen and tumid.

There is no sternal or pectoral deformity, but the supra-clavicular regions are both full, without yielding any unnatural pulsation. The heart is rather large, and at its base is a rough double murmur, which can be traced upward to a point of maximum intensity over the junction of the right second rib and sternum. No fullness or pulsation in supra-sternal notch. There is slight hoarseness. Many of these points were determined at a second examination.

Taking into consideration the place of beginning of the pain, its distribution, and its typically neuralgic nature on the one hand, and on the other hand, the absence of pain near the seat of intra-thoracic disease, I made the double diagnosis of cervico-brachial neuralgia, and aneurism of the innominate artery. Contrary to what Mr. S.' former medical advisers had said, I did not believe that his neuralgia was a sympathetic or reflex pain dependent upon the aneurism, and hence incurable. I thought the coincidence a fortuitous one.

Acting upon this belief, I at once began treatment by cauterizing the neck with Paquelin's instrument, giving a deep injection of morphia over the brachial plexus, and ordering 4. of Thomp-

son's solution of phosphorus (equal to about .003 of phosphorus) to be taken every three hours. In forty-eight hours very great improvement had occurred; no pain in the head since cauterization, and only two paroxysms in the shoulder and arm. Several cauterizations were made; morphia given by the mouth for a few nights; the arm was kept quiet. In about two weeks iodide of potassium was substituted for the phosphorus, and quinine also given. The neuralgia had almost ceased by the end of June; but the whole arm felt queer, heavy, and swollen (semi-painful); the radial artery was smaller, the substernal dulness more marked, and the double murmur over the innominate artery louder. In other words, while the cervico-brachial neuralgia was nearly cured, the aneurism was making progress.

Occasionally, there was slight return of cervico-brachial pain.

On August 23d, Prof. Austin Flint corroborated the diagnosis of aneurism of the innominate artery, and suggested a trial of Tufnell's rest and low-diet treatment. This the patient has decided to submit to. He has no neuralgia (none to present date, Sept. 8th); he is troubled by a severe cough, with bronchial catarrh; his right arm is puffy and bluish, and feels badly. The local physical signs in the chest are the same. He takes quinine after breakfast, and a gramme of iodide of potassium in infusion of digitalis four times a day.

I am led to publish this case because of the belief that had I looked upon the neuralgia as sympathetic and expended my therapeutic efforts upon the aneurism as the *fons et origo mali*, I should have also failed to relieve the patient.

NOTES ON A CASE OF ACUTE PHTHISIS, SHOWING THE FUTILITY OF HEROIC ANTIPYRETIC TREATMENT.

By R. W. AMIDON, M.D.

The patient, a girl of twenty-two, was admitted to the New York Hospital, in the service of Dr. Woolsey Johnson, October 12, 1878.

Without heredity or antecedents, she gave the history of having contracted a severe cold six months previously. Since that time, pain in the chest, cough with greenish expectoration, dyspnœa, anorexia, and emaciation made the diagnosis and prognosis only too clear.

A physical examination revealed signs of consolidation at both apices, with fine râles at both, and cavernous râles at the left apex.

The respiration was 26, the pulse 96, and the temperature 38.6° C. (101.6° F.).

The patient was simply put to bed, given some cough mixture, and on the following morning the pulse was 76, respiration 25, and the temperature 37.4° C. The daily examinations for the next twenty days revealed a continued fever, with an average morning and evening temperature of 38° and 39° respectively, a pulse of 85-105, and a respiration of from 20-30.

A first attempt at heroic treatment was made Oct. 4th, with the following result :

OCT. 4th.	T.	P.	Resp.
7 A. M. . . .	33.3°	100	23
11 A. M. . . .	38.9°	105	22
Cool bath . . .			
Immediately after bath . . .	40°	108	34
11.40 A. M. . . .	39.8°	103	32
4 P. M. . . .	39°	95	30

The bath was faithfully administered in the same way, and of the same temperature as those which, in our typhoid fever cases, produced such good results, and such sudden and lasting lowering of temperature. But in the present case, it was so disagreeable to the patient, who was not in that stupid, unresisting state common in typhoid fever, but on the contrary wide-awake to every thing, as hectics are, and, moreover, as it was productive of no good results, speedy or permanent, it was never repeated.

For about three weeks, only ordinary remedies were used. High fever, with non-periodic chills at intervals, continued. Occasionally a normal temperature was reached in the morning, but generally the record was :

	T.	P.	Resp.
A. M. . . .	37.3° - 38°	75-85	25
P. M. . . .	39° - 40.5°	105-118	35-44

Oct. 27th, another attempt was made to break the fever.

	T.	P.	Resp.
9.30 A. M. . . .	37.9°	100	23
Adm. salicylic acid, 5.20			
11 A. M. . . .	38.2°	74	27
1 P. M. . . .	38.2°	74	21
9 P. M. . . .	39.5°	108	32

This single large dose of the acid, although producing no disagreeable symptoms, had little effect on the rise of temperature, except to postpone it until later in the afternoon. Exhibition of the drug in equal amount, but in divided doses during the morning, was but little more effective.

Another interval of fifteen days passed, palliative remedies in ordinary doses being given.

Meanwhile the disease, as shown by physical signs, had made rapid advances. The patient ate little or nothing, had lost two kilos. in weight in six weeks, and was never free from chest pains and cough except when under the influence of morphine.

On Nov. 5th the temperature attained, in the afternoon, the height of 40.5°, and had stood above 39.5° since 9 A.M.

Nov. 11th.—New antipyretic means were employed.

NOV. 11TH.	T.	P.	RESP.
3 A.M.	38.4°	92	24
Quinine sulphate I.			
7 A.M.	38.2°	84	24
Quinine I.			
9 A.M.	38.4°	95	24
Quinine I.			
11 A.M.	39.4°	95	28
1 P.M.	39.7°	104	24
9 P.M.	38.5°	90	24

Considerable headache, vertigo, tinnitus, and deafness ensued. The temperature attained the same height as on the two preceding days.

In a few succeeding days, while taking only simple remedies, the patient complained much of headache and photophobia, and was very irritable.

These symptoms passed off, and Nov. 17th, 18th, and 19th the patient was very comfortable, in spite of a temperature of 40.4° in the morning, and 40.1° in the afternoon of Nov. 19th.

Nov. 20th, another trial of quinine was decided on.

NOV. 20TH.	T.	P.	RESP.
I A.M.	38.4°	102	28
Quinine .60			
2 A.M.	38.3°	84	32
Quinine .60			
3 A.M.	37.1°	76	28
Quinine .60			
4 A.M.	37.3°	76	28
Quinine .60			
6 A.M.	37.1°	79	Not recorded.
Quinine .40			
8 A.M.	37.2°	76	"
Quinine .40			
2 P.M.	37.1°	79	"
4 P.M.	37.3°	80	"
Quinine .30			
6 P.M.	37.8°	72	"
8 P.M.	38°	80	"
10 P.M.	38.5°	80	"
Quinine .60.			
NOV. 21ST.			
I A.M.	38.4°	92	"
Quinine .60			
2 A.M.	38.5°	89	"
Quinine .60			
4 A.M.	38.4°	80	"
Quinine .30			
9 A.M.	37.4°	85	"
Quinine .60			
10 A.M.	38.1°	73	"
Quinine .60			
11 A.M.	38.3°	89	"
Quinine .30			
1 P.M.	38°	84	"
Quinine .60			
2 P.M.	37.8°	82	"
Quinine .60			
6 P.M.	38.3°	89	"
9 P.M.	38°	75	"
Quinine .30			
10 P.M.	38.3°	62	"
Quinine .30			
NOV. 22D.			
3 A.M.	37.3°	67	"
Quinine .60			
4 A.M.	37.6°	72	"
Quinine .60			

Nov. 22D.	T.	P.	RESP.
5 A.M. Quinine .60	38°	67	Not recorded.
9 A.M. Quinine .70	37.6°	80	"
10 A.M. Quinine .60	37.4°	74	"
3 P.M. Quinine .60	37.8°	75	"
4 P.M. Quinine .60	38°	80	"
5 P.M. Quinine .30	38.6°	96	"
10 P.M.	37.3°	65	"

On Nov. 23d, the patient complained so much of the headache, deafness, etc., that no quinine was administered, the amount of 16.5 grams having been taken and retained during the three preceding days.

Nov. 24th. The headache was very severe.

Nov. 25th. The patient was very restless. She was crying and moaning with the headache all the time. An intense photophobia caused the patient to roll up her eyes to such an extent that the pupil could hardly be exposed. To-day, for the first time, noticed a hesitancy and deliberation in her speech.

Nov. 26th. Although crying a good deal, slept nearly all day under the influence of frequent small doses of chloral, the morphine which she took before not agreeing with her.

Since stopping the quinine the temperature had been gradually rising till to-day it attained the height of 40.6° at 3 P.M.

Nov. 27th. The patient to-day scowls, bores the pillow with the head, complains of pain in the back of the neck, which is very tender, and dysphagia. There is present partial paralysis of the left side of the face, and slight divergent squint when the eyes, which are still rolled up, can be seen. Vital signs: T. 40.3°; P. 132; Resp. 26. Administered during the morning iodide of potassium, 2.40.

Nov. 28th. The former symptoms continue, in addition to which the patient complained of general hyperæsthesia and diplopia. Gave iodide of potassium, 3.60 at 9 A.M., 3.60 at 2 P.M., and 6. at 7 P.M.—13.20 grams in ten hours. Vital signs: T. 40.6°; P. 127; R. 33.

Nov. 29th. A little better if any thing, but weak. Ordered milk punches and iodide of potassium, 6.50 in the morning, 7.20 at noon, and 7.20 at night. Vital signs: T. 40.7°; P. 180; R. 30.

Nov. 30th. The patient was collapsed and thought to be dying in the early morning. She rallied about 8 A.M., after the free administration of brandy by the rectum and hypodermically; marked photophobia and hyperæsthesia with intense headache continued. There were also marked divergent squint and occasional clonic spasm of the facial muscles of both sides. No iodide was given in the morning because of the vomiting, but at both one and six P.M. 7.80 were given. The maximum temperature to-day was 40.5° ; P. 180; R. 25.

Dec. 1st. The patient has great tenderness in the abdomen, which is tympanitic, and pain in the ankles and knees, the legs being drawn up. There was involuntary evacuation of urine. The sacrum, which has been reddened and sensitive for a week, was now excoriated. The strabismus was less. The iodide was given morning and noon in 7.80 doses; none at night. Ordered 7.80 of the iodide three times a day. The vital signs were: T. 40° ; P. 178; R. 32.

Dec. 3d. There was present to-day no headache, no facial palsy, little photophobia, and no strabismus; but the diplopia persists. The vital signs for the day were: T. 39.5° ; P. 120; R. (not recorded).

The iodide was continued in 7.80 doses three times a day till Dec. 10th, when all cerebral symptoms had ceased, and decided iodism had developed.

All active treatment was now abandoned, and the pulmonary symptoms, which were in abeyance during the cerebral complication, again became prominent. The patient lingered for two months longer.

The only cerebral lesion found on autopsy was a thickening of the pia-mater at the base of the brain.

This case shows the impotency of our so-called antipyretics in combating the pyrexia of phthisis.

In rheumatism, salicylic acid, and in malarial fever, quinine lowers the temperature, because they neutralize the poison or destroy the germ on which the pyrexia depends.

In acute phthisis, however, besides a doubtful septic factor, we have a local destructive inflammatory process, which, unlike many of our other inflammations, shows little or no tendency to self-limitation, spontaneous or induced cure. When, therefore, the diagnosis and prognosis are clear, in our present ignorance, antipyretics hold out no hope, and the expediency of their use at the expense of the personal comfort of the patient is doubtful. One or two trials of them, in the hope that an error in diagnosis has

been made, or that good in some way may accrue, is justifiable, nay, perhaps wise. Their persistent use, however, should be condemned.

A study of the above case emphasizes some of the physiological and therapeutical effects of the means employed. Some of them we will enumerate.

On the day the cold bath was given 1302 cc. of urine were secreted, against 930 cc. the day before and 558 cc. the day after. The bath was followed, immediately, by a rise in the temperature (not uncommon), pulse and respiration as usual; and, more remotely, by a fall of all three vital signs. The day on which a single large dose of salicylic acid was given was also marked by polyuria, and the usual mild cerebral symptoms. The antipyretic effect of the drug was only manifested by a postponement of the afternoon rise of temperature, pulse, and respiration until 9 P. M.

In reviewing the effects of quinine on the patient, a more interesting and perplexing field is opened. The first time when 3. of quinine was given in six hours there followed the usual headache, vertigo, tinnitus, and deafness, all of which were transitory. There ensued a few days of inexplicable headache, photophobia, and irritability, which also vanished, but were admonitions of what was coming.

November 20th, 21st, and 22d the patient took 16.5 grams of quinine. During these days, and on the 23d, the common sensory physiological effects were very marked. The febrile temperature and pulse also yielded in a very marked way (see table). On the 24th the headache became intolerable; and later, undoubted indications of a cerebral lesion presented themselves: headache, pain, and tenderness in the back of the neck, general hyperæsthesia, partial opisthotonos, dysphagia, photophobia, strabismus, diplopia, facial palsy, hesitancy and deliberation in the speech, the formation of a bed-sore, and, finally, complete disappearance of the cerebral symptoms under large doses of the iodide of potash.

Most probably the pathological condition in this instance was a meningitis. Then the question arises whether, in a patient subjected to conditions like ours, the meningitis was tubercular, or that rare and much tabooed form which many authorities claim can come from large doses of quinine.

We are inclined to the latter view for the following reasons:

Tubercular meningitis, if *ever* cured, would doubtless leave behind some local lesions in the meninges, while in this case they were diligently sought for and not found.

No history of syphilis could be proven. Large amounts of quinine, however, had been taken, and doubtless caused the diffuse, basal meningitis, whose only trace on *post-mortem* was a diffuse but decided opacity of the basal pia mater.

HYSTERICAL CONVULSIONS AND HEMI-ANÆSTHESIA IN AN
ADULT MALE: CURE BY METALLO-THERAPY (GOLD).

By E. C. SEGUIN, M.D.

Examples of hysterical convulsions in the male sex are rather frequent in youth and boyhood, but after twenty they become so rare as to be worthy of record. Still more unusual is it (in this country at least) for hemi-anæsthesia to follow the succession of convulsions. For these reasons, and because the case presents points of interest as regards diagnosis and therapeutics, I desire to place it before the readers of the ARCHIVES.

James A., 21 years old, single, and a laborer by occupation, was brought to the Manhattan Eye and Ear Hospital by Dr. Smith of Newtown, Ct., for the diagnosis and treatment of an alarming set of nervous symptoms; briefly summed up as convulsions, extreme staggering, left-sided hemi-anæsthesia.

History.—A year ago the patient fell from the upper platform of a freight car, a distance of at least ten feet, striking the ground upon the back of his head. He thinks that he was unconscious for a few moments, but did not vomit. Remained well after this fall until some six weeks ago. Denies sexual excesses or irregularities. At that time, some six weeks ago, he had an ill-defined illness—apparently a severe “cold,” characterized chiefly by pains all over his body, in the muscles mostly. Thinks that he had no fever (locality is malarious, however), and is positive that he had no articular swelling or sore throat. The account of the order of appearance of the nervous symptoms is obscure, as Dr. Smith did not see patient until two weeks ago. Then had already had several “fits,” apparently of an epileptic nature; he was not paralyzed, but exhibited complete insensibility to pricking on the left side of his head, face, tongue, and body. He also staggered somewhat. He complained of headache, near the vertex, and over the right parietal region. Convulsions occurred every night; and one night about ten days ago there were several, which were witnessed by Dr. Smith. In these attacks the patient was stiff; eyes closed, showing, when the lids were raised, normal pupils;

the respiration was slow and gasping ; the spasm was only tonic, and lasted, quite certainly, not less than three minutes. There was no frothing of the mouth, or subsequent drowsiness. The patient claimed not to know any thing of these seizures. Attacks occurred yesterday. A friend of the patient describes attacks lasting an hour and a half. The staggering gradually increased during the fortnight of observation ; clear (colorless?) urine was often voided ; no globus or emotional seizures. Has seemed rather obtuse or stupid. Much bromide of potassium has been administered ; at first he had forty grains (2.75 grammes) every four hours, and later every two hours, and less often. Altogether, has taken about 45 grammes ($\frac{5}{8}$ jss) in ten days.

Examination.—Patient is an average, dull-looking Irishman, generally pale, and with the neurotic white circle about his mouth strongly marked. Comes into the room supported by two persons ; staggers preposterously ; when not supported plunges off to one side or the other. No paralysis ; sees and hears well (to simple tests). Pupils normal. Left side of body, face, and tongue presents complete analgesia. Ends of fingers are a little sensitive to *deep* pricking (only in last two or three days). The various modes of sensibility and the special senses were not critically studied, because we purposed doing this on another day. To watch-test and to ordinary objects there was no deafness or blindness of the left ear and eye. An interesting experiment was made upon the patient as regards his equilibrium. I placed him in the middle of the room, loosened his friend's hold of his arm, and told him to look up at the ceiling and try to see certain fine marks upon it. Thinking that I was testing his eyesight, he strongly directed his attention that way and stood *perfectly well*, without a trace of his staggering ; which, however, returned the moment that the test was over and he was told to stand alone—that he could not do (while thinking of it).

Without saying any thing to the patient or to the physicians and students standing by, I applied two twenty-dollar gold pieces to the patient's left hand, and afterward to his forearm, cheek, and tongue. I most positively said or did nothing which could *suggest* any thing to him. He could not tell whether I meant this as a continuation of the examination, or as a remedial measure ; he looked and spoke as if he thought I was amusing myself in applying the gold.

In a few moments, one to three minutes, sensibility returned in each spot where the metal had been laid ; completely so in the

tongue, and partially in the cheek, forearm, and hand. The patient was amazed. I ordered a capsule containing .30 of citrate of iron and quinine and .01 of extract of nux vomica to be taken four times a day.

The next day, September 21st, the patient was examined by my colleague, Dr. W. R. Birdsall, who found him nearly free from staggering; pricking with a needle was felt a little less than normally on the left face, quite normally on the left arm and hand. On the left leg (not yet treated) pricking was somewhat felt, but simple touch was not perceived. A belt of gold plates was applied round about the calf for ten minutes, when sensibility was found to be restored, not simply where the plates had been laid, but throughout the extremity. On neither day was any phenomenon of "transfer" observed.

On Sept. 23d patient, claiming to be perfectly well, left the hospital contrary to my request.

The diagnosis of the case presented but slight difficulties, in spite of its extreme rarity. The staggering was evidently overdone, or at least greater than in any organic or functional cerebral disease known to me; and it was made to cease by diverting the patient's attention in an interesting manner. The convulsions were too long to be any thing but hysterical, and the state of the pupils indicated the non-epileptic nature of the seizures. The continuation of the symptoms—nay, their aggravation—under severe bromide treatment was in accordance with my own experience in hysterical cases.

The brilliant success of metallo-therapy in this case is interesting and very puzzling. I believe that every physician present when the gold was first applied, will agree with my statement that there was no sort of *suggestion* made to the patient—nothing was said until after the patient himself looked up in amazement at the restoration of sensibility to his hand and tongue.

It might be added that in the last two years I have had several successful cases of metallo-therapy in my practice—all of them reactions to gold. One of the most striking was that of a girl aged about sixteen, showing decided chloro-anæmia, but free from hysterical symptoms except analgesia of the whole left upper extremity, and the neurotic white circle about the mouth. In this case an elongated oval area on the extensor surface of the forearm remained analgesic in spite of several short applications of the gold, but yielded to their continued contact for twenty-four hours.

ARCHIVES OF MEDICINE.

Original Articles.

A CASE OF MYXŒDEMA.

BY ELIZABETH M. CUSHIER, M.D.,

PHYSICIAN TO N. Y. INFIRMARY FOR WOMEN AND CHILDREN.

Mrs. S., æt. fifty-seven. Born in England, but has lived in this country since infancy. Has had twelve children, her last pregnancy being eighteen years since. Health had been good until she received a severe shock during N. Y. riots in '61. She was at that time living in a part of the city where there was a large Irish population, and her husband being known as a Republican and Abolitionist, his life was threatened and their property destroyed. From this time her health began to fail. She lost strength and suffered from various anomalous ailments. Within the next few years—a precise date could not be obtained—her friends began to notice a change in her face, which became gradually swollen. She also grew inactive, from having been of an energetic habit. This inactivity she attributed to a difficulty in walking. She felt it a burden to walk, and had a sensation when attempting to do so of being drawn forward. Her appetite remained good, and with the exception of what appeared to be a slight deafness, her faculties remained unimpaired. I saw the patient for the first time in the winter of '77. Was called on account of œdema of feet and legs, and various new and distressing symptoms. I found her sitting up in a chair with her head hanging forward. Her face presented a strange appearance: it was large, especially about the cheeks; the nose was flattened, making distance between eyes appear greater, and it was also thickened; the curve of the nostrils was lost; the lips were thick, and the lines about the mouth were obliterated; the eyelids were puffy and bagging, and the eyes, owing to the thickness of lids, seemed small and narrow. The general appearance of the face suggested

the œdema of Bright's disease, although there were differences which became later more marked. There was at this time œdema of lower extremities, and to a slight extent also of hands. Bright's disease was, therefore, suspected, and an examination was made of the urine. The utmost care, however, failed to detect the slightest evidence of renal disease. The urine was absolutely normal. An examination of other organs, which was carefully repeated in the hope of finding some cause for the existing condition, gave negative results; with the exception of a slight increase in the area of the heart's dulness, nothing was found.

The cause of this œdema remaining unexplained, the patient was treated symptomatically, and under the use of diuretics, with strychnine and iron, the swelling gradually diminished, and finally disappeared, with the exception of that of the face, which remained in the same condition. There was but little change in the general symptoms of the patient, and she remained comparatively comfortable until the summer of 1879, when the œdema of lower extremities returned, and she had an acute attack of inflammation of skin of posterior aspect of legs. The spots were circumscribed and vesicular, and they rapidly ulcerated, and as rapidly healed, under treatment which consisted mainly of rest, bandaging, and astringent lotions. From this time—with the exception of an intercurrent attack of hemorrhoids, which were operated upon and very readily healed, nothing of special note occurred until a short time before patient's death. During this time the difficulty in walking and supporting herself in a sitting posture increased, and she remained for the most part in bed. The inability to rise or walk was peculiar. The patient would wait for some seconds after the impulse had apparently arisen, as if preparing herself for the effort. She would then very slowly rise, supporting herself by her hands while doing so; her head would remain bent strongly forward until she had risen, then by an effort she would slowly lift the head and stand erect, with the head, however, always inclining somewhat forward. When once upon her feet, and started, patient could walk, and there was no peculiarity about her gait other than its slowness and clumsiness.

There was no apparent diminution of general sensibility in extremities, at least none that could be detected without the use of the æsthesiometer, and the œdema quite disappeared only to return a few weeks previous to her death. During these years the skin had become very dry and harsh, and the face still more enlarged, and as the increase in size did not involve the forehead,

the comparative narrowness here, together with absence of expression, gave the countenance a strange and stupid aspect. The cheeks were firm and waxy to the touch, and rosy pink in color forming a marked contrast to the transparent whiteness of skin around mouth and eyes. The lips had entirely lost their mobility, so that when patient spoke it was as if through a mask.

The case remained an enigma to me until about fourteen months before her death. At about this time my attention was attracted by an article by Dr. Clifford Mercer, of Syracuse, on "Myxœdema."¹ The paper in question was read before the Syracuse Medical Society, and published later in the *Medical Record*. Dr. Mercer had seen, through the kindness of Dr. Ord, of London, some cases of this disease—then entirely new to him,—and his description of them, and later Ord's account,² left no room for doubt as to the nature of the trouble in my patient's case, and a careful examination for further details only corroborated the diagnosis. There were the same semi-transparent skin; the bagging of eyelids—hanging, indeed, in large folds,—the even and persistent flush on the cheek, terminating by a sharp line at lower border of orbit, and forming a marked contrast with white circular area surrounding eyes; the flattening of nose, and increase of width between eyes; the placid face, with thick expressionless lips of a violet tint; thickening of tongue, and fat bolsters in superior clavicular region; slowness of comprehension, and slowness of response; labored articulation, and monotonous voice. All were there, making a most complete picture. Added to this were the difficulty in rising and walking, without paralysis, and also the hanging of head upon chest, above mentioned, and attributed by Ord to difficulty in co-ordination and to incapacity of will to maintain muscular action. The patient was, as in Ord's cases, well nourished. The mental processes were correctly performed—but slowly, and with an effort,—and

¹ *Medical Record*, April 16, 1881.

² *Tr. Clin. Soc., London*, 1880, xiii.

finally there was a subnormal temperature and slow pulse. Having decided upon the nature of the case, I asked Dr. Putnam Jacobi to see the patient with me. The doctor entirely agreed with me in the diagnosis. The subsequent history of the case is briefly told.

The symptoms mentioned became more marked, and the patient was confined constantly to her bed. Her appetite remained good, but there was obstinate constipation. There were occasional attacks of œdema of lower extremities, which, however, subsided, and only became permanent a few weeks before patient's death. The peculiar condition of the skin characteristic of the myxœdema, was limited to the face and neck; elsewhere there was no permanent swelling, and none that would not—like the true œdema—pit on pressure. The skin, however, was everywhere—excepting the face—dry, harsh, and scaly. The heart's area became markedly increased, and percussion during last few months of life gave abdominal dulness, which changed with change of position of patient, and gave rise to supposition of ascitis. The abdomen also greatly increased in size. About three weeks previous to death, patient had an acute attack of facial dermatitis. It began on the left cheek and extended across nose to opposite cheek, and later to both ears. The swelling was very great, and there were vesiculation of the skin and considerable pain. A diagnosis in regard to the character of the inflammation was quite difficult, as the general appearance resembled in many of its characters an erysipelatous inflammation. The almost complete absence of fever and other constitutional symptoms determined the exclusion of erysipelas, and the rapid course and termination of the disease proved the correctness of the diagnosis. A few days previous to patient's death there were an acute swelling and increase of temperature of left upper arm, with pain about elbow joint, and also a small phlegmon on outer side of right breast. An interesting point in this connection is that the patient could not locate the pain, but constantly referred it to her right arm. This was the only evidence of any disturbance of sensation. The patient's strength from this time gradually failed, and she died on the 12th of June, 1882.

An autopsy was held fourteen hours after death with the following results :

Rigor mortis well marked. Body moderately fat. Abdomen

quite prominent. Slight œdema of lower extremities. Face large and waxen. Eyelids transparent and bagging. Skin with exception of face rough and scaly.

The spinal cord was first removed. Upon cutting through integument of cervical and upper dorsal regions it was found to be very thick, and the subcutaneous connective tissue infiltrated with transparent material, giving it the appearance of the umbilical cord. This condition was not found elsewhere in any part of the skin which was divided, but it probably existed in the face. Nothing abnormal was found macroscopically in the spinal cord. An attempt was made to examine the brain, but owing to an excessive thickening and hardening of the bones of the skull, and the unfavorable conditions under which the autopsy was made, the patient being in an ice-box, the attempt was reluctantly but necessarily abandoned. Upon opening the abdomen the walls were found to be very fat, and it was noted with surprise that no fluid was present in the cavity. The omentum and mesentery were loaded with fat, and it was to this that the dulness on percussion was due. The laxity of the abdominal walls, and the weight of the fatty intestines favored the descent of the latter toward whichever side upon which patient was placed, and gave rise to the absolute dulness on percussion, and to the change of the same on changing the position of patient. The thyroid gland was somewhat smaller than normal, especially the left lobe. The latter had undergone cystic degeneration. The right lobe was of unusually firm texture. The pericardial sac contained a slight excess of fluid. The heart was enormously increased in size, both ventricles were hypertrophied, and the right also somewhat dilated. The tissue was pale and firm. The valves were normal. There was beginning atheroma of aorta. The lungs were quite normal, and there was no effusion in the pleural cavities. The spleen was normal. Liver was normal in size, but tissue was unusually firm. The kidneys were slightly enlarged, and there was moderate atrophy of cortical portion.

The following tissues were subjected to microscopic examination, and with accompanying results: Skin and muscle from dorsal and anterior cervical regions, portions of tongue, kidney, spinal cord, and thyroid gland. The liver was not examined, owing to the specimen having been mislaid.

Microscopic Examination.

The subcutaneous connective tissue was found greatly increased, and there was atrophy, and in part complete absence,

of the hair follicles, and sebaceous and perspiratory glands. The individual fibrils of connective tissue appeared swollen, and there were scattered here and there groups of small round cells. The nuclei of connective tissue were distinct and large. In many places the tissue presented a glistening whiteness, and there were also present small spherical bodies which had a highly refractive power. Most of these were entirely homogeneous, but others contained finely granular matter.

The muscular fibres of heart were markedly atrophied, in many places being diminished to one third their original size. The striæ were almost obliterated, and there was a diminution in number of nuclei. There was an increase of the interfibrillar connective tissue, making wide spaces between the fibres, and these spaces were filled with small glistening spherical masses.

The kidneys presented, both in the medullary and cortical portions, atrophy of the tubes and Malpighian bodies, with an increase of the interstitial tissue. In isolated places there was complete disappearance of the tubes, and a substitution of swollen connective-tissue fibres. Here, also, were found the same transparent, white, highly refractive masses noticed in sections from heart and subcutaneous tissue. The epithelial elements of kidney were granular. In the thyroid gland the walls of alveoli were thickened, and their contents had either entirely disappeared or were replaced by a homogeneous substance. The smaller blood-vessels were mostly obliterated by the excessive thickening of their coats, caused by infiltration of hyaline substance between the fibres forming them.

There was also an increase of connective tissue, presenting the same swollen appearance, in the tongue. In places there were single muscular fibres widely separated from the remaining bundle by this interfibrillar substance ; also, large irregular-shaped spaces filled with a hyaline substance.

In the spinal cord changes were found in the blood-vessels and in the nerve cells. In many of the sections the small vessels had undergone changes in their walls, resulting in a thickening and infiltration of the same, with a transparent homogeneous substance. In the lower third of the lumbar cord, in region of anterior horn, was a mass composed of dilated blood-vessels and granular pigment. In

other portions of lumbar region, also in columns of Clark and in lower cervical region, slight hemorrhages had taken place, in some places the pigmentary remains of which could alone be seen. There was marked dilatation of blood-vessels throughout most of sections.

In the sections from lumbar cord, in both anterior and posterior horns, there were here and there cells which had undergone complete "yellow degeneration." There were also atrophy of the cells, and absence, or diminution in the number, of their processes. In posterior columns outside of horns, there were small spaces filled with a transparent, glistening material. In upper portion of lumbar cord the atrophy of cells was less marked, but many of them were changed in character. The central canal was obliterated.

In cervical cord, lower segment, the cells in anterior horns were more numerous than in the sections below, and the atrophy of cells in posterior horns less marked. There were also in the white columns numerous glistening spaces, and on outside of the columns, small round glistening bodies. In upper segment of cervical cord the changes were still less marked, but the cells were still diminished in number, and the atrophy of nerve tubes gave mottled appearance to sections. There was here also partial obliteration of central canal, Throughout dorsal region changes similar to those above were noticed.

I am indebted for the above results of examination of spinal cord to my friend Dr. Putnan Jacobi, and also to the kindness of Dr. Birdsall.

Since 1873, when Dr. Gull read before the Clinical Society of London, a paper entitled "A Cretinoid Condition Supervening in Women in Adult Life," up to the present time, about fifty cases of the disease in question, now generally known as myxædema, have been reported. The name given to it by Dr. Gull was suggested by several points of

resemblance in the physical character and mental symptoms of the disease, to cretinism, and it was not until six years later that any explanation of its pathology was offered. At this time Dr. Ord, of London, published a report of three cases,¹ in one of which there had been a *post-mortem*, and to him is due the interesting discovery, that the œdema was the result of an infiltration of the connective tissue with a substance containing an enormous amount of mucin.

While these investigations were being pursued in England, another observer; Dr. Morvan, in Basse Bretagne, had noted a peculiar affection occurring in women, and of which he could find no description. In 1875, he wrote to Charcot, giving an account of his cases—the description of which leaves no doubt as to their nature,—and asked for some light upon the subject which was puzzling him. After some little time had elapsed, he received word from Charcot, that neither from his own experience nor from the careful study of the most recent literature could he find any thing which would correspond to the condition in question, and he concluded by advising Morvan to publish an account of his cases. This Morvan deferred, while he continued to observe and record his cases, until in 1881 his attention was directed to an article by Merklen, in the May number of the *Gaz. hebdomadaire*, in which a case of myxœdema was described. Morvan at once recognized the disease as being identical with that which had for so long been an enigma to him, and in the following August he published a paper in the same journal, giving a detailed account of fifteen cases which had come under his observation in Basse Bretagne.² Among these cases (which occurred, as in the experience of other observers, mostly in middle-aged women,)

¹ Trans. Clin. Society, London, 1878, lxi, 57-78.

² *Gaz. hebdomadaire*, Paris, August, 1881.

were three under the age of twenty-five years, and one case occurred in a man. With one exception, the subjects of the disease were peasants. In the meantime, cases had been reported by Charcot¹ in Paris, Duckworth,² Lloyd,³ and Inglis,⁴ in England, and Clouston⁵ in Scotland; while in this country one case had also been presented by Dr. Hammond.⁶ Since that time still other cases have been brought into notice, until, so far as can be ascertained, there are now, including the one above reported, in all, fifty cases on record. It is singular that no mention of the disease—at least by any recognizable name—is found in German medical literature. A careful search in the different German journals has been absolutely without results.

In all the cases reported the general aspect of the patient is remarkably similar, especially in regard to the appearance of the face, scaly condition of skin, absence of hair, and suppression of perspiratory secretion. There is also a universal agreement in the history of the cases in regard to slowness of speech, muscular weakness, and slowness of all muscular acts. The slowness of mental processes is almost invariably admitted, but in regard to mental disturbance other than this, there is great diversity in the statements of the different cases. Doctors Gull and Ord lay especial stress upon the fact, that all the mental acts are generally accurately performed but with unusual slowness. Dr. Morvan also in his fifteen cases reports the intellect intact with the exception of two cases. In both of these there were hallucinations, and in one of them the intellect was enfeebled. The following table will give, as far as could be ascertained, the symptoms pertaining to the nervous systems in the various cases which have been reported.

¹ *Progrès Méd.*, Paris, 1880, Nos. 30-31.

Gaz. d'hop., Paris, 1881, 1-in, 73-75.

² *Lond. Lancet*, 1879, ii, 577.

Med. Press and Circ., London, 1879, n. s., xxviii.

³ *Lancet*, London, 1881, i, 138.

⁴ *Lancet*, London, 1880, ii, 496.

⁵ *Edinburg Med. Journal*, 1880-81. ⁶ *Neurological Contrib.*, vol. i, No. 3.

	CASES.	SEX.	AGE.	SPEECH.	MUSC. SYMPT.	CEREBRAL SYMPT.
Gull	5	Female	45	{ Guttural and labored	Musc. languor	{ Unimpaired intellect. Mind indifferent. Mind placid and lazy. More irritable.
		"	40		Slow movements	
Ord	6	"	54	Slow	Loss of power	{ Strange taste. Weak-headed. Languor. Slow perception.
		"	36	"	Slow action	Slowness of thought. Ideas clear.
		"	40	"	Slow movements	Slowness of thought.
		"	45	"	" "	" "
		"	60	"	" "	" "
		"	52	"	Staggering gait	Memory defective.
Morvan	15	Female	55	Slow	Enfeeblement	Halluc. and enfeebled intellect.
		"	27	"	Gen. paresis	Intellect intact.
		"	44	"	Slow movements	Hallucinations.
		"	48	"	Enfeeblement	{ Has observed no diminution of intelligence in his cases.
		"	40	"	Gen. paresis	
		"	50	"	" "	
		"	22	"	" "	
		"	46	"	" "	
		"	40	"	" "	
		"	40	"	" "	
		"	46	"	Enfeeblement	
	3	"	43	"	"	Lethargy
		"	39	"	"	
		"	67	"	"	
		Male	50	"	"	Intelligence intact.
Fournier	1	Female	55	"	"	Hallucinations in dreams, but intellect intact.
Duckworth	3	"	47	"	Slow movements	Dull and morose.
		"	46	"	" "	
		"	34	"	" "	Aberration.
Charcot	3	Male	57	"	"	Apathy.
		Boy	19	"	"	
Lloyd	2	Female	40	Thick	Slow movements	Delusions.
		"	65			
Inglis	2	Male		Slow	{ Slow movem'ts Dimin. farad. react.	Mind enfeebled.
		Female	39			Delusions. Silly. Childish.
Thaon	1	"	39	Slow	Movements slow	{ Frightful dreams. Slow thinking.
Mahomed Lattey	1	"	30	"		
	1	"	57	"		Dull and heavy.
Blaise	1	"	34	"		{ Halluc. of taste, smell, hearing, sight. Slowness of thought. Apparitions and frightful dreams during sleep.

	CASES.	SEX.	AGE.	SPEECH.	MUSC. SYMPT.	CEREBRAL SYMPT.
Hopkins	I	Female	49	Slow and thick	{ Slow movements Sometimes fell Dimin. elect. react.	Memory impaired. Temper milder. Deafness. Taste and smell impaired. Sight dimmed by mists and showers of specks.
Gowans	I	"	60	Slow	Staggering gait	{ Slowness of thought. Mental indifference. Memory defective. Irritable and depressed at times.
Hammond	I	Female	41	Slow and sluggish	Staggering gait Dim. of cutaneous sensibility	Diminution of sense of hearing. Abolition of sense of taste and smell. Hallucinations. Failure of memory, and marked weakening of intellect.
Cushier		Female	50	Slow and labored	Loss of musc. power. Walked with effort	Deafness. Slowness in responding when spoken to; intellect otherwise intact.

In addition to above, other cases have been reported by Simon Carafy, and Fenn, but precise statements in reference to the nervous symptoms were not obtained.

The various opinions in regard to the essential nature of the disease in question are based in part upon the theory that the main anatomical lesion, *i. e.*, the mucoid infiltration, is the point of departure for the nervous symptoms, and in part upon the theory of a profound general nutritive disturbance, of which the connective-tissue change is but the manifestation.

Ord, who was the first to recognize the important anatomical change which had taken place in the connective tissue, points to the analogy existing between this general condition and that of the embryo, where the elements of connective tissue, especially those containing mucin, are predominant, and speaks of it as a reversion or degeneration toward the embryonic state. He then argues that such a

condition of the skin, by interfering with the natural exposure of nerve-ends to stimuli, appears in itself an explanation of most, if not all, of the characteristic nervous phenomena above related. And as one most essential condition of health of the central nervous system is, that it receive its due amount of stimulation through skin, and receive it regularly, any such padding of peripheric nerves would result in the perception becoming slower than usual, and thus the central nervous system losing its natural stimulus, a state of intellectual lethargy and slowness in co-ordination would be the result. In his first two autopsies Ord did not find any corresponding changes in the nerve tissue. Later he admits these changes, but regards their effects as secondary to the constant, extensive, and progressive interference with function of peripheric nerves.

Fournier,¹ after analyzing the symptoms in the recorded cases, thinks the mental manifestations null in comparison with the muscular paralysis, which is present in every case observed, the importance of which is as marked as the œdema itself, and remarks that the paralysis affects all the muscles, the striated as well as those of vegetative life, and as a result we see fatigue in walking, defect in equilibrium of body, difficulty and slowness of speech, dysphagia, constipation, slowness and feebleness of heart's action and of arterial pulsation; and he concludes that the pathological process is evidently an intimate alteration of the nutrition of the cutaneous envelope and subjacent cellular tissue; and as the histological perversion is general, it is necessary to seek its cause in the acts which regulate the nutritive movements in the affected parts.

According to Morvan,² in whose detailed account of fifteen cases there is a marked predominance of muscular paresis over the mental symptoms, the motor nerves are

¹ *Gaz. hebdomadaire de médecine*, Paris, 1882, 2 s., xix.

² *Loc. cit.*

paralyzed, and myxœdema is a neurosis of the nervous system, affecting the system only in the parts which pertain to the motor nerves,—as well those of animal as those of organic life,—and leaving unimpaired all those pertaining to sensation and the mental faculties.

Hadden,¹ after noting the diminution of temperature and diminution of urea observed in all cases where these conditions were sought, sees in some disturbance of the vaso-motor system an explanation of most of the symptoms of myxœdema, that is, diminished tissue-waste, diminution of heat, together with bodily and mental sluggishness, and asks in this connection: "If vaso-motor paralysis be the cause of the enlarged thyroid in exophthalmic goitre, may not vaso-motor spasm be invoked on behalf of the opposite condition of this gland in myxœdema?" He mentions, in this connection, a case of Dr. Ord's, in which exophthalmus precedes the onset of the disease, and says that a certain degree of protrusion of the eyeballs has been observed more than once. Dr. Hadden then offers the following explanation of the solid œdema: "Angiospasm does not affect the blood vascular system alone, but involves also the lymphatic. Under such conditions, there would be a generalized lymphatic obstruction, and an inability to remove the products from the lymphatic spaces," and adds, that "it is conceivable that these accumulated products should, in the course of time, undergo changes resulting in the formation of mucin."

The most recent paper on the subject is one by Henrot, which was read before the recent meeting of the "Association française pour l'avancement des sciences." Henrot gives a case in which a most careful *post-mortem* was made, and the following are his conclusions: "The disease is characterized by a generalized muroid œdema of the subcutaneous

¹ *Brain*, vol. v, No. 2.

cellular, and submucous tissues; by a mucoid degeneration of the upper and lower extremities, tongue, lymphatic glands, liver, spleen, great sympathetics, pituitary body, and pineal gland; by atrophy of the muscular tissue of the heart, and blood-vessels, and of the voluntary muscular system, also those parts of the nervous system which preside over sensation and intelligence; by a profound anæmia giving to the skin the appearance of wax without the existence of leucocythemia. In a word, the vegetative centres take an absolute preponderance over the centres of animal life."

Henrot places the lesion which produces this general mucoid degeneration in "*l' appariel cinerio hypophysaire*, which is formed at the same time by a vascular gland and the anastomosis of the two trunks of the great sympathetic." He then draws attention to the study of Owen, according to whom this body is markedly developed in the embryonic state, and also in animals who, like the fish, have normally a large amount of mucoid matter, and concludes: "The disease known as myxœdema, or cachexie pachydermique, seems essentially constituted of a return of the subcutaneous, submucous, and interstitial connective tissue, to the embryonic state, under the influence of hypertrophy of the vegetative centres, and particularly of the sympathetic ganglia and the vasculo-sanguineous glands, which are especially connected with them."

In regard to the light which has so far been thrown upon the nature of the disease in question, Ord's discovery alone, based as it is upon existing anatomo-pathological conditions, is of any scientific value. The theories in regard to its essential cause are unsupported by facts, and are in part hypotheses having no known physiological data from which to infer the presumed pathological change. Until, therefore, further investigation has brought more precise knowl-

edge from which to draw conclusions, the disease in question can only be described as a nutritive disturbance resulting in the presence in the connective tissue of a substance common in embryonic tissue, but not existing normally, excepting in very small amounts, in adult life, at which time the connecting substance of connective tissue is formed largely of collagen. As these two substances, collagen and mucin, are, it is presumed, the result of decomposition of the proteids, it would seem that the process in myxœdema is one in which the nutritive material intended for the repair of the connective substance, is converted into a lower form of tissue, one which is a reversion to an embryonic condition, whether or not at the expense of the collagen has not, that I am aware, been ascertained. The association, as in the above case, of colloid with the mucin is not surprising when the chemico-physiological relation of the two substances is taken into consideration.

The points of interest in the case recorded above are, the limitation of the mucoid œdema of the skin to the face and neck, while the muscular paresis of the lower extremities, mentioned as a prominent symptom of the disease, was very marked. At the same time, the sections from spinal cord gave evidence of changes which must certainly have been connected with this loss of muscular power.

Whether in these cases the paresis is due to a primary lesion in the spinal cord, some form of myelitis to which the hyperæmia and degeneration of cells would point, or whether the essential cause is a trophic neurosis, of which the changes in the cord are but one manifestation, upon which, indeed, they depend, are questions which at present cannot be answered; but the identity of the changes in the blood-vessels, in the cord, and in those of the other tissues, and the presence in its connective tissue of the same abnormal products which are found elsewhere in this tissue, would

point to the latter as more probable, and suggest for the disease in question a relationship to those other troph-neuroses—progressive muscular atrophy and pseudo-muscular hypertrophy,—in which the main anatomical lesion, at least in the early stages of the disease, is found in the connective tissue.

In the case reported above, to whatever may have been due the changes found in the spinal cord, they were certainly sufficient to account for the muscular paresis, leaving no need for evoking a peripheric cause.

POSSIBLE CONTAGION OF PHTHISIS.

By E. G. JANEWAY, M.D.,

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AS the possible origin of tuberculosis and phthisis by contagion has become the subject of experiment and study on the part of members of the medical profession, and of increasing importance to the public, I have thought that the following possible instances possessed sufficient value to warrant their publication.

The first is one of exceptional interest, as it shows that dogs in contact with a man suffering from phthisis became affected with a pulmonary complaint ending in death. Moreover, it is a confirmation of those experiments made upon dogs to elucidate this difficult problem. They were made to inhale an atmosphere containing the sputum of tuberculous patients distributed by a steam atomizer. (See *Virchow's Archiv*, vol. 74, p. 393, not vol. 48, as given in Clapp's book.)

The patient, a young man twenty-three years of age, had lost his mother by consumption. Two years before the onset of his fatal illness, he had suffered for four months from what he considered a cold, which left him without a trace. I first saw him in Sept., 1879, complaining of a cold, which had developed five months previously. The disease was, however, a spotted tubercular trouble of a considerable portion of the upper lobe of the left lung, and to a less extent of the right lung. The symptoms indicated a graver disturbance than the physical signs considered

alone would have justified. On a subsequent visit he brought a pet dog with him, which excited my interest by the cough which troubled it from time to time. On inquiry he narrated the following: This was the third dog that he had owned since his sickness. The two predecessors had died after becoming affected with a cough which very closely resembled his own, and that during the paroxysms they at times, as he himself, vomited. All of these animals had been well at the commencement of his proprietorship in them. It was his habit to take the dog to bed with him, and to sleep with it nestling in his arms, its face and snout being turned toward him. In this position it of necessity inhaled his breath, and the atmosphere evolved during his coughing spells, which, at times, were frequent during the night. The first of the dogs was a black-and-tan terrier; the next a King Charles spaniel; and the third a Scotch terrier, if my memory serves me. It has always been a source of regret, that he was unwilling to part with his last dog in the interest of scientific observation and a solution of this vexed problem. The young man died, Mar., 1880, but this last animal survived him, though troubled with a cough and having lost flesh and strength.

I have been tempted to repeat the experiment thus performed by this patient, but have not deemed it wise to do so in my hospital service, and in private practice I have not met the right patient. Some of the readers may find the means for the repetition before long. I would suggest that care be taken to select a vigorous animal, and to give it sufficient out-door exercise to maintain its health. If possible, I should advise sacrificing it if sickness should occur, instead of permitting a natural death.

The following clinical cases have some interest also.

Nearly ten years ago I saw a young married woman who was affected with a slow pneumonic infiltration of one lung. The disease began in the upper part of the lower lobe, and gradually spread until the lung became consolidated at the end of seven weeks. During this whole time the only sputum was that ordinarily characterizing pneumonia. Then the process remained nearly stationary for a year. At the expiration of this time she became much fatigued in consequence of a long journey, and in

three weeks rapid ulceration and breaking down of the lung terminated in death. Her father was ill with pulmonary phthisis in the same house for some time previous to her sickness. Her husband, a physician, acquired a cough, as he supposed, from exposure to cold in making the arrangements for her funeral. But this was but the inception of a fatal phthisis of the more ordinary variety.

Another group comprises a mother, daughter, and son.

I saw the mother and daughter but once, in consultation with the attending physician, Dr. M. Fleming. The family had no inherited tendency to phthisis. The girl had been affected with hysterical paralysis and other manifestations of this protean disease. A physician who saw her had advised her going to St. Luke's Hospital. While there she was treated in a general ward, in which, according to her account, phthical patients had their beds. At the time of going she was stout and plump, but shortly began to lose flesh. She remained until December, and then returned home. Soon after she began to have a cough. At the time of my visit, in the middle of May, 1880, she had a large cavity, with thin walls, in the upper lobe of the right lung. Her mother at this time, aged fifty, had spots of tubercular disease in the right upper lobe, some of these being in a state of incipient softening. In her case she had been taken with a cough some time in December. During the autumn she had been considerably prostrated in strength, in consequence of nursing a daughter through typhoid fever, contracted in the Catskill Mountains. Later a son, who at the time of my visit was well, also contracted the disease, and within a few months of one another they succumbed to the malady.

Some of the value of this case is destroyed by a doubt which attaches to the priority of the disease in the mother and daughter. In May the physical signs showed far more advanced trouble in the daughter, but the information about its time of inception of the cough in the two is indefinite. The daughter, however, ran down rapidly before leaving hospital, and before cough was a notable event. The case of the son presents a greater probability of contagion.

Still another recent observation is of interest.

R. S., who died Feb. 23, 1882, of phthisis after a two years' illness, was during the last three months confined to his room. After his death the room was occupied by his brother-in-law and his wife. On May 2, 1882, I saw this gentleman in consultation, he having acute phthisis of the left lung, with a good deal of pneumonic infiltration and rapid ulcerative disturbance. The disease began, according to his account, about five weeks after his brother-in-law's death.

I would add to these the following groups, seen within the last three months.

A young man, engaged to a girl in whose family there was no phthisical taint, became consumptive and died. She was much with him during his illness, and shortly before his death became affected with a cough, which later terminated in death. Her sister, who was much with her during her illness, soon became affected with a cough, which proved chronic, for it had lasted six years at the time of her visit to me during an exacerbation. This was due to a new extension of the disease to the upper part of the left lung, it having been previously confined to the right, as I learned from friends' account of previous medical examinations.

The next group comprises five persons.

The first, a young man sixteen years of age, died of phthisis after several months' illness, four years ago. A sister, aged twenty-two years, who had been much with her brother, after the lapse of a year showed evidences of the disease, and died two years ago. The mother then became ill, but there was an apparent interval between the daughter's death and the commencement of her sickness of some months. Yet, as her daughter says, she had a wheezing in her chest before her sister died, for which she used poultices; it is not improbable that the disease was present already. She died August 5, 1882. The first that I saw of the family was when called to examine one of the three surviving daughters, eighteen years of age. She had had repeated hemorrhages from the lungs for three weeks, and on examination showed evidences of phthisis at the apex of the lung. Whilst writing this account still another sister has called on me to ask what her sister had better do, and, as the result of a request on my part that I might examine her lungs, I find that she has decided

trouble at the right apex, though ignorant of it. These two sisters were much with their mother while ill. There was no hereditary tendency to phthisis on either side of this family.

In addition to these, I have lately examined a young woman who became ill with her phthisis shortly before her mother's death from this disease. Recently, at Bellevue Hospital, a woman ill with consumption had just become affected at the time of her husband's death from this malady. A gentleman a few days since complained of his misfortune in losing the services of a valued clerk, owing to his having fallen a victim to phthisis. On inquiry he said that the man had been closely attentive to his wife, who had recently died after a two years' illness.

The reader can find a number of striking cases collected in Dr. De Musgrave Clay's little work, published in Paris in 1879, or a 'smaller series, mainly from this source, in the work by Clapp, of Boston.

I add these cases to those which have been published by others, hoping that they may draw attention to this matter, which is one of great importance. I know that many people can be exposed for long periods of time without injury. But the question is, does phthisis spread by the reception of tubercular poison (*bacillus*?) by those favorably disposed to it, who would otherwise have escaped? In the wards which I visit at Bellevue Hospital are subordinates and attendants who have been there for years, notwithstanding that numerous phthisical patients have lived and died there. This, however, proves nothing more than that those persons have not taken the disease. As an illustration of the want of weight which should be accorded to these negative facts I will use the following illustration:

Some soldiers go through a campaign without injury, whilst others are killed. One might say that bullets, etc., will not kill, because these soldiers escape. Moreover, the

disease is slow in its progress, and it is only after a considerable time has elapsed that one can trace the connection; and when several members of a family have died in succession, this is explained on the basis of an hereditary taint. Every physician must have noticed the strong predisposition which some individuals exhibit to diseases known to be contagious. So much so that they at times fall victims, whilst the cause of the poison escapes. I saw this exemplified in the late outbreak of typhus in this city. The physicians under the Board of Health who examined the cases, the physicians who had charge of the cases, the ambulance drivers who removed them, escaped. Cases were for a time in Bellevue, Charity, Roosevelt, Mt. Sinai, St. Vincent's, the German, and other hospitals, yet only three physicians in this city and Brooklyn contracted the disease; one each at Bellevue, the Homœopathic, and at Flatbush hospitals. These physicians were not as much exposed as others.

In one instance a nurse, who had charge of several cases in one family, where the disease had been contracted by two of the children standing near an ambulance, which removed a person sick with typhus, without having the disease herself, conveyed it to a man who occupied a room belonging to the family with which she boarded. She slept during the day on a lounge, on which this man would lie when he came home tired from his work. An attendant in the Small-pox Hospital, contracted the disease, though she had not been in contact with a patient, or the clothing of a patient. She simply came in contact with others who had attended patients sick with typhus, though these did not sicken with the disease. I knew of but one case occurring in the better-to-do class of people, and that was in the person of a merchant whose store is on Broadway. He said that he had seen no one sick with the disease, allowed no

peddlers or tramps in his store, always rode in the elevated cars to his home, which was in a part of the city where the disease did not prevail. He contracted the disease by a minimum of exposure at an unknown time, and in an unknown manner. The nurse who took care of him and the physicians who visited him, did not contract the disease, though unprotected. I have chosen typhus fever as an illustration because of its admitted contagiousness, and of its usual requirement of rather prolonged exposure, to show in a clear light the necessity of a third factor beyond the individual and the poison, which, for the lack of a better name, I call predisposition, or, better still, a suitable condition of the system. It may be that with phthisis to a greater extent than with typhus, a prolonged exposure acts only by allowing the poison of the disease to take root, because at some time during this prolonged exposure, it meets with the favorable conditions for its growth. Had I not made careful inquiry, I should have remained ignorant of the majority of the facts here presented. I presume that the hurry incident to practice prevents many physicians from becoming cognizant of similar instances. These cases and others recorded in the literature of the subject have impressed me sufficiently to lead me to give some words of caution upon the continuous use of the same room, and to prohibit in the main the use of the same bed. Also to insist upon those waiting upon phthysical patients having plenty of out-door air and exercise. As regards the phthisis being conveyed to dogs, I believe that amongst the people there must be a superstition, that if the disease can be transmitted to an animal then the person having it may escape. At least I have been asked on several occasions questions which seemed to point in that direction.

THE ETIOLOGY OF CONGENITAL TALIPES EQUINO-VARUS.

By H. W. BERG, M.D.,

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LITTLE as we know concerning the etiology of disease in general, this knowledge will appear relatively extensive, when compared with what is known of the etiology of congenital diseases and deformities. This is probably owing to two reasons: 1st. The difficulty of discovering any cause of disease in a stage of life which ought to be purely physiological and normal; and 2d, because the conditions of fœtal life are especially difficult of investigation. It is not, therefore, surprising that congenital talipes equino-varus, although so common a deformity, has its etiology still unsettled. Nor is this owing to a lack of study of the deformity; for the frequency of its occurrence, together with the seeming simplicity of the subject, has attracted all orthopædic writers, so that there are few who have not either advocated one of the more or less generally accepted theories, or advanced a new one.

To attempt a review of all of these, with the arguments for and against them, would be to perform an almost useless task. For many of these theories are mere theories, having no foundation in well-observed facts. But, on the other hand, we can not properly proceed to the study of the subject, without first becoming familiar with, at least,

the opinions held at the present day by leaders in this department of surgery.

Orthopædic writers may be divided into two classes as regards their views concerning the causation of congenital talipes equino-varus.

1. Those who consider the deformity as the result of a mechanical force acting upon the fœtus in utero.

2. Those who consider it the result of disease (intra-uterine), whether of the osseous, muscular, or nervous systems.

Let me first consider the mechanical theory. This was far more popular among older writers than among those of the present day. It still, however, has prominent advocates; among whom may be mentioned Parker, of London,¹ Vogt,² Volkmann,³ Luecke,⁴ Banga,⁵ and Kocher.⁶ By these authorities it is assumed that, for various reasons, the foot, in certain cases, is maintained in one fixed position, by the pressure of the walls of the uterus, for so long a time that it becomes permanently fixed in this position. This theory would thus account for all the forms of club-foot, and this general applicability is its only merit. For, apart from the fact that the fœtus in utero is not an inert mass, but a living being, which moves about in its surrounding fluid, and does not maintain its feet in any fixed position, the theory has this objection, that other parts of the body, which, under similar circumstances, should be subjected to a similar pressure, are not found, with equal frequency, to have acquired this false position. Permanently flexed knee-joints, for instance, should be, according to this view, a very frequent instead of the rarest possible deformity. Indeed this should be a more frequent deformity than club-foot; for this abnormal pressure, which is supposed to take place when the liquor amnii is small in amount, must be equal on all sides of the fœtus, so that those joints to which

are attached the longest levers should be most cramped. Were the mechanical theory true, "clubbed" legs and "clubbed" thighs should be more frequent than "clubbed" feet.

This theory is supposed to be strengthened by the fact that fixation of a joint for a lengthened period produces a limitation of the joint's motion. This is certainly so ; but in club-foot we have nothing similar to ankylosis. There are no adhesions to overcome. On the contrary, motion as far as is possible in the deformed position of the joint, is good, and as soon as we rectify the deformity by tenotomy, the motion of the joint becomes unrestricted.

The second class of theorists consider the deformity as due to some pathological change affecting the child in utero. To this class belongs the large majority of authorities upon this subject. The secret of its popularity lies in the fact that there are many points of similarity between congenital and non-congenital cases of club-foot ; so that it is tempting to ascribe to the former the well-known etiology of the latter. " Really," said Little, as long ago as 1839, " we can clear up the etiology of congenital club-foot by comparing it with that of non-congenital cases. These are caused, first, by paralysis ; second, by reflex contracture." And in 1853⁷ he again expressed the opinion that congenital club-foot was due to a " disturbance or disease of the nervous system."

Following the lead of this distinguished pioneer in orthopædic surgery, we find most of the succeeding writers ascribing congenital club-foot to one or more of the causes which are known to be active in the production of non-congenital cases. The fallacy of such reasoning is evident when we consider the various possible pathological causes in some detail.

First, then, a portion of the older writers held that con-

genital talipes equino-varus was due to a primary displacement of the bones, resulting, secondarily, in a tonic contracture of the muscles, whose points of origin and points of insertion had been approximated. Hence, an operation on the muscles was not indicated. But as soon as dissection had demonstrated that, while old cases of congenital club-foot, which had used the deformed feet in walking, did indeed show considerable displacement and deformity of the bones, in the new-born infant with club-foot such displacement was not seen, it was understood that in the former cases the displacement of the bones was due to the use to which the deformed limb had been put. Stromeyer recognized this, but went even further and considered the muscular contracture as the primary condition in the pathogenesis of the deformity. In this he was a disciple of Delpech,⁸ who taught that a muscle could be the seat of permanent spasm, and thus disturb the relation of bones to each other. We now know that this view is faulty, and by no one was this so well shown as by Volkmann.⁹

He proved, without doubt, that tonic retraction of healthy muscles does not occur, whether the opponents be healthy or paralyzed; and further, that the structural shortening of the muscles is due, not to contracture, but to growth while the foot remains in its deformed position. However erroneous the opinions of Stromeyer may have been, they still led to highly beneficial results. For it was the conviction that the muscles were the primary cause of the deformity that led him to perform the operation of tenotomy, the benefits of which in the relief of various deformities have been so invaluable to the human race.

Prof. A. Luecke⁴ has advanced an original theory, which places him among those who consider the osseous structures of the joint as the seat of primary lesion in club-foot. This writer has observed the fact that at a certain period of life

in utero all fœtuses are club-footed. He considers that this early deformity is counteracted by the constant movement of the child in utero, which serves to grind off the articular surfaces of the joint in such a manner as to enable the child's foot to assume the normal position before birth. "If, however," he reasons, "the movements of the fœtus are restricted from any cause, this attrition of joint-surfaces will not take place, and the child will be born deformed." However ingenious this theory may be, it is not very tenable, for granting that at a certain stage of fœtal life the joint-surfaces are opposed to each other at such an angle as to cause the feet to be "clubbed" (which has not been proved by dissection), then surely motion in the deformed position will only serve to increase the faulty direction of the articular surfaces. Indeed, it is motion and use of the deformed feet which make them worse after birth; why should the same mechanical force be followed by a different result during fœtal life? But apart from this we know that in non-congenital cases want of use does not so change the articular surfaces as to cause permanent club-feet. In spastic paralysis, for instance, when the feet have been held in equinus for years, tenotomy is followed, temporarily, at least, by a return of the foot to its form and function. This fact is stated by Dr. Paul Ruprecht, of Dresden,¹⁰ who has had much experience in tenotomy for this cause. I should not have drawn particular attention to this theory, were it not that Prof. Luecke's paper recognizes two important facts:

1st. That the fœtus at a certain period of its existence is club-footed.

2d. That children born with talipes equino-varus have a deformity of the whole extremity, although he has radically mistaken the character of this deformity. The importance of these two facts will be seen later on, when we develop our view of the etiology of club-foot.

In thus discussing the theories which make the bony structures the primary seat of deformity in this form of club-foot, we have unavoidably mentioned those by which the muscles, apart from their connection with the nerves, are considered as the determining factor in the etiology of this affection. No one can suppose that the contraction of the fibrous tissue around the joints—plantar fascia,—ligaments, etc., is any thing but secondary, for being non-contractile (in an active sense) they merely undergo stretching or structural shortening to suit any position of the joint which is long maintained.¹¹

It is therefore only left us to review those who account for this deformity by a disturbance or change in the peripheral or central nervous system. Among these are included Barwell,¹¹ who believes talipes varus to be due to a want of development of the motor tracts; Broadhurst,¹² Little,⁷ Maligne,¹³ and Bonnet (de Lyon),¹⁴ according to whom this deformity is due to some functional or organic disease of the nervous centres; Rudolphi,¹⁵ and Louis Bauer,¹⁶ who rather incline to the belief that a functional disturbance of the nervous system, perhaps reflex in character, can cause a "spasmodic action" of certain muscles, which, they claim, should it continue for even a short time, while growth is going on in the healthy opponents with the rapidity which is characteristic of this stage of life, would soon disturb the even balance between the opposing groups of muscles, thus rendering the deformity permanent. In addition to these authorities, we have Wm. Adams,¹⁷ who discusses all of the principal theories of the etiology of club-foot. He considers that the theory of arrest of development, which we shall take up separately, has been satisfactorily disproved by dissection, nor does he believe in the mechanical theory, but rather vaguely seems to give his adherence to the theory of the nervous origin of club-foot, admitting the possibility of

a paralytic affection of the muscles (ant. tib. group) in early foetal life, giving rise to this deformity. Prof Sayre¹⁸ devotes very little space to the discussion of this question. He thinks, however, that most congenital cases are paralytic in their origin.

A classification of these various opinions upon a pathological basis, would be as follows :

1. That club-foot may be caused by paralysis or spasm of a set of muscles due to cerebral disease.
2. That it may be caused by paralysis or spasm of a set of muscles due to disease of the spinal cord.
3. That it may be due to a reflex disturbance of the nervous centres.

Taking up the first, we find that cerebral lesions may cause deformity in two ways :

a. By the mere paralysis of muscles (deformity resulting from their inactivity—Volkman⁹).

b. By the secondary contracture which follows paralysis of cerebral origin.

Were the deformity of talipes equino-varus or simple varus due to a cerebral lesion and paralysis resulting therefrom, we should expect to find not a single group of muscles but a whole side paralyzed ; which certainly is not the case in children with congenital club-foot, apart from the fact that congenital talipes occurs so often in both legs. As for a local lesion of the cortex, such as a premature ossification of certain parts of the skull, causing pressure and producing this deformity, the fact just stated, together with the healthy condition of most of these children, is opposed to any such theory. We have here none of the symptoms of cerebral disease, such as convulsions, exalted reflexes, etc.

A very commonly assigned cause has been a lack of development of a portion of the motor tracts of the brain. This is accompanied by a secondary lesion of the lateral

columns of the cord, and that portion of the anterior columns designated as the columns of Tuerck, and gives rise to a condition known as spastic paralysis (Erb) or tetanoid paralysis (Seguin),¹⁹ in which talipes equinus and sometimes equino-varus is the deformity of the feet. In addition to this is cited the fact that the anacephalus monsters, and children with hydrocephalus, have so often been found to be club-footed.¹¹ But in all of these cases the deformity is due to contracture of groups of muscles, and we have not only club-foot but a general rigidity and spastic condition of the muscular system, an exaltation of all reflex action, and an evident lack of mental development varying in degree from only a slight amount of slowness in cerebral action, to utter absence of intellect as in anacephalus monsters. But surely these children cannot be likened to the intelligent and otherwise healthy children with congenital club-foot, in whom there is no disturbance of the nervous system; while objectively, the foot has a different appearance from that in cases of a spastic nature. The principal point of difference being a peculiar movement of the toes in cases of the latter class, somewhat resembling the athetoid movements of the fingers described by Hammond,²⁰ which is never present in congenital club-foot. Finally, to reason that because talipes occurs with the above conditions, these are necessarily the cause of all cases of congenital talipes, is as illogical as it would be to consider that since abdominal pain occurs with peritonitis, all abdominal pain is due to peritonitis.

Paralysis due to disease of the cord is a fruitful cause of non-congenital club-foot, hence the frequency of the opinion, that this is the most probable cause of congenital cases. In children the affection of the cord most commonly productive of paralysis, is poliomyelitis anterior, and to this disease can be traced a large majority of the non-congenital

cases of club-foot of every variety. It is well known that after this paralysis has lasted for a certain time, contractions of opposing and non-paralyzed muscles take place, and the foot assumes a deformed position. As to the mechanism of the production of this deformity I would again refer to the excellent paper of Volkmann⁹; but the fact that it does take place is undisputed. And what can be more tempting than to suppose that congenital club-foot is due to a similar inflammation of the anterior horns of the spinal cord? But a careful examination of the condition of the muscles paralyzed by poliomyelitis anterior, compared with that of the muscles which should be assumed to be the seat of paralysis in congenital club-foot, does not support any such theory.

First, a mere objective examination of a case of congenital talipes and one of infantile paralysis reveals various points of difference. In the first the leg is fat, well nourished, and of normal surface-temperature, showing no signs of the muscular atrophy and cold surface which are characteristic symptoms of the second. Again, in many cases of infantile paralysis other muscles besides those of the leg are the seat of paralysis, while in congenital club-foot this is never the case. Lastly, the electrical examination shows a radical difference. Muscles paralyzed by anterior myelitis do not react to the faradic current, while the galvanic reaction is altered in quantity or quality, or both. I have carefully examined six cases of congenital talipes equino-varus, all under sixteen months of age, and found normal faradic reaction, while there was no change from the ordinary formula to the galvanic current, although it was somewhat difficult to pick out the individual muscles, and the quantity of the reaction was slightly diminished. But certainly in all of the muscles the closure of the negative pole produced a greater contraction than that of the positive. The above

cases had not yet begun to walk, and were therefore best fitted for examination.

It might be claimed, however, that the regenerative power of the foetus is so great as to produce a recovery from the lesion in most cases, so that after birth the neural symptoms above stated would be absent, although the temporary paralysis had been sufficient to fix the foot in a deformed position. But when we consider that contracture does not take place in poliomyelitis anterior until months after the paralysis, and that, should recovery take place early, as often happens even after birth, the contracture would not occur, we will readily understand that the deformity cannot be explained in this way. A more extensive study of this subject will lead to the conviction that no lesion similar to poliomyelitis anterior is the cause of congenital club-foot.

Finally, we come to consider the possibility of this congenital deformity being due to a spastic contracture of the muscles, either from disease of the spinal cord or of reflex origin. It is well known that, theoretically, a primary lesion of the lateral columns of the cord (crossed pyramidal tracts) should produce a spastic condition of the muscles obtaining their nervous supply from that portion of the motor tract in which the lesion is located, and I presume that it is to such a lesion that the present upholders of this theory would refer some cases of congenital club-foot. But it is by no means generally acknowledged that a primary sclerosis or degeneration of the lateral columns of the cord ever occurs in children; and even were such a condition possible, the cases would resemble those of spastic paralysis due to secondary degeneration of the lateral columns, which we have already discussed, and to which, as we have shown, children born with club-foot bear no resemblance.

For the same reason it is necessary to exclude from the etiology of congenital talipes a similar spastic condition,

supposed to be the reflex expression of some distant pathological condition of the general system, even were we inclined, in the light of recent discussion, to acknowledge the possibility of such a reflex spastic paralysis.^{21, 22}

In concluding this review of the most important of the existing opinions concerning the etiology and pathogenesis of congenital talipes equino-varus, which I considered necessary to a proper understanding of the subject, I would call attention to the fact that none of the foregoing theories very well conform with the acknowledged hereditary element in the etiology of this affection. The mechanical theory, its principal point being an accidental position of the fœtus in utero, certainly does not; and were we to acknowledge any disease of the nervous system as the cause, the laws of heredity would lead us to expect the appearance of other nervous diseases in the same family, in addition to the one causing the deformity in one or more of its members.

An examination of the family history of the last forty-six cases of congenital talipes equino-varus which have appeared in Dr. N. M. Shaffer's service at the Orthopædic Hospital, shows only one case in which a neurotic element occurred in the history, and this was an "anterior myelitis" affecting another child of the same family. In two cases there was a family history of phthisis. In the remaining forty-three cases the family history was healthy.

All of the theories hitherto discussed, with the exception of the mechanical theory, have considered this congenital deformity as the result of a pathological process. I shall endeavor to present, in a new light, a view which, although feebly advanced by some authorities from time to time, has never been supported by well-observed facts, and has, therefore, gained no adherents. This theory supposes that the deformity of pes equino-varus is due to an *obstruction in the normal development of the feet, during some period of*

intra-uterine life. This would seem, at first, the most natural direction in which the thoughts of the inquirer concerning this subject would turn. For in this way we can account for many congenital deformities, such as hair-lip, spina bifida, congenital cysts, congenital herniæ, etc. But analogy only hinted at a similar etiology for club-foot without proving it, which was not enough to force conviction, even when combined with the negative proof that all other theories were insufficient.

It has only been in the last few years that the study of human embryology has advanced sufficiently far to enable us to bring positive arguments in favor of this theory of obstructed development; which, as we shall see, does not necessitate, as Adams¹⁷ seems to have thought, any radical change in the individual bones and muscles which form the club-footed extremity.

It is not surprising, then, that after an extensive search in the latest literature of the subject I found no use made of this advanced embryological knowledge to establish the etiology of talipes equino-varus. But lately, in consulting Billroth²³ upon some other subject, I happened to turn upon the chapter on the etiology of club-foot, and found that he strongly expresses his convictions in favor of this latter theory, as explained by recent embryological data, although he does not clearly define either the theory itself or the data upon which it rests..

The fact that in early fœtal life (sixth to seventh week) the sole of the foot is turned in, in the varus position, was acknowledged by many of the earlier writers, such as Isidore Geoffroy St. Hillaire,²⁴ and Wagner,²⁵ although so little was known concerning human embryology, that even this fact, now so well established, was denied by such authorities as Cruveilhier,²⁶ Malgaigne,¹³ and Little.⁷ But those who had observed this fact, considered that an arrest of de-

velopment at this early stage (third to fourth month, Wagner; sixth to seventh week, Geoffroy St. Hillaire) would perpetuate the early foetal position of the foot.

In exactly what changes the normal process of development consisted, and how the foot gained its normal position, they did not understand. It will here, then, be instructive to study the main facts in the process of the development of the lower extremities, dwelling upon those points only which are of importance in the present discussion.

As early as the third or the beginning of the fourth week of the life of the human embryo, we can distinguish four small budding projections, which correspond to the future extremities of the foetus (Kölliker,²⁷ Barwell,²⁸ Dalton,²⁹ Gray³⁰). These projections or tubercles are due to an increased rapidity of growth over a small, limited area of the external blastodermic layer or epiblast, thus forming an external fold, into which shoots a projection from the outer lamina of the mesoblast or somato-pleura, from which the voluntary muscles of the chest and abdomen also originate. At first these tubercles project horizontally from the body, but as they grow they adapt themselves to the curve of the body of the foetus.³¹ Their relative position at this early period does not exactly correspond to that of the upper and lower extremities later on. For the upper pair of tubercles are relatively low down, about the middle of the foetus, while the lower pair are relatively high up. But as the embryo progresses, the cephalic and caudal ends grow less than the intermediate portions, and the extremities come to occupy their relatively normal position. The excellent plates of the foetus at this early stage, in Kölliker's work, already cited, as well as the rather diagrammatic representations of the development of the chick embryo in the works of His and Forster and Balfour, will illustrate my remarks.

For our purpose it will be unnecessary to study the origin of the component tissues of these tubercles or embryo extremities. It will be sufficient to follow the gross, objective changes which these extremities undergo in their development, and this study can be best carried on by a comparison of specimens of the human foetus at different stages of embryonic life. This I have been, to some extent, enabled to do by means of the specimens of the human foetus at the New York Hospital Museum, aided by a study of the authorities upon the subject.

At about the fifth week the clefts between the fingers, and later, those between the toes, are formed, and we are enabled to see that the extensor surface of both limbs looks upward and corresponds to the dorsum of the foetus, while those which will be, later on, the radial and tibial borders are directed toward the cephalic extremity of the embryo, (specimens 877, 1351, and 1527, N. Y. H. M.), and the works of Prof. Humphrey³¹ and Albert Kölliker.²⁷

At about this period a differentiation takes place along the axis of the rudiments of the extremities, and a mass of the original tissue or "blasteme," as it is called in the translation of Kölliker's²⁷ work, is converted into cartilage with its perichondrium, thus forming a continuous cartilaginous skeleton for the extremities (Kölliker²⁷ and Barwell²⁸). Soon, however (sixth or seventh week), shallow grooves begin to appear externally, marking the position of the future joints, the distal segments being first distinguished. The first appearance of a joint is seen soon afterward, when whitish lines appear, crossing the cartilage opposite these grooves, and by a process of softening and liquefaction of the original cells, according to some authorities, among whom is Barwell, the joint cavities are formed. Here again the most distant joints (those of the phalanges) are formed first, the others following in the reverse order of their proximity.

Kölliker is uncertain whether the joints are formed by a process of softening (*ramolissement*), and we cannot here enter upon the views of different authorities upon this point, but he agrees with the opinion that the skeleton of the extremities is originally one continuous cartilage, and that as the joints are formed, it becomes divided into the different segments of the extremity. The first joint appearing from the sixth to the eighth week, and the last at the end of the third or beginning of the fourth month.

If, then, we examine a fœtus at the beginning of the third month, we find that the elbow and knee joints have already been formed. But the extensor surfaces of the upper and lower extremity no longer look in the same direction; for, whereas before both patella and olecranon were directed backward, and later on outward, now the knee is directed outward and principally *forward*, while the elbow is looking outward and principally *backward*. That is to say, the extremities have altered their primitive position in some manner, and assumed the one which they maintain through life. The rotation at this early period is not yet complete, but soon becomes so in the ordinary process of development.

This movement is not characteristic of the human fœtus, but has been observed in many of the vertebrates. Forster and Balfour,³² in their researches on the embryo of the chick, found that although at first the angles of both the joints, corresponding to the knee and elbow, look outward and somewhat backward, yet on the eighth day the elbow looks directly backward and the knee forward. Prof. Humphrey,³¹ who has carefully studied this rotation of the extremities, describes it in the following manner: "In the primitive position the extensor surface of the two limbs is directed upward or dorsally, the radial and tibial borders being directed forward. In this position the two extremities are

homologous in their parts. Subsequently, by a quarter turn, to some extent participated in by the pelvis, the extensor surface of the hind limb is directed forward, and the tibial border now looks inward. By a quarter turn in the opposite direction, to some extent participated in by the shoulder, the extensor surface of the upper limb is directed backward, the radial border therefore looking outward. At the same time pronation occurs in the forearm and hand, whereby the palm is brought in contact with the ground." This view is the one generally adopted, although Dr. P. Albrecht, of Kiel,³³ holds a somewhat different opinion, into which we cannot now enter, and in answer to which Prof. Humphrey wrote the article from which I have quoted.

The cause of this rotation which, as I shall soon show, is of the greatest importance in the study of the etiology of congenital club-foot, is unknown. Kölliker remarks that it cannot depend upon muscular power, for the muscles have not yet appeared when this rotation has begun. He thinks, however, that the phenomena of growth which take place in the members themselves as well as in the adjacent regions, play an important part in this rotation.

The movement does not take place suddenly, but beginning early, at about the second month, is to a great extent accomplished by the fourth month; but not completed until the fifth and beginning of the sixth month.

A study of the specimens in the New York Hospital Museum, as well as those in the Wood's Museum of Bellevue Hospital, will show the progressive changes in the position of the extremities at different periods of foetal life. These specimens were examined in the glass jars in which they were mounted.

As will be seen from the table, not only is the leg as a whole rotated outward during early foetal life, but this outward rotation is accompanied by an exaggerated varus, and

NUMBER OF SPECIMEN.	MUSEUM.	AGE.	APPEARANCE OF EXTREMITIES.
877	N. Y. H. M.	4 weeks	No joints apparent in limb buds. Extensor surface of both limbs (upper and lower) directed dorsally. Limbs project perpendicularly from body of fœtus.
1351	N. Y. H. M.	6 weeks	Peripheral joints and those at knees, apparent. Limbs applied to sides of fœtus. Extensor surface of both limbs directed outward.
884	N. Y. H. M.	2 months	All joints present; lower extremity rotated <i>outward</i> . Thighs flexed upon abdomen; legs at about right angles with thighs, crossing each other and applied closely to body of fœtus. Knees look outward. Feet in <i>equino-varus</i> position. Soles on a concave line with inner border of leg. Arm rotated inward. Radius directed toward the head.
1472	N. Y. H. M.	3 months	Leg still rotated outward. Knee looking outward and very slightly forward. Feet in exaggerated varus position. Abdomen very protuberant.
1201	Wood's M.	4 months	Legs flexed upon thighs at less than right angles. Inward rotation of thighs about half complete. Feet in equino-varus position; inner border raised, and deeply concave so as to conform with outline of abdominal wall. Elbows directed outward.
1203	Wood's M.	5 months	Inward rotation of legs very far advanced. Knees look forward and slightly outward. Varus of feet slight. Equinus no longer present. In upper extremities elbows look more backward, but here, as in lower extremities, right side appears further advanced in rotation than left.
1204	Wood's M.	6 months	Rotation complete. Knees look forward. Feet almost as straight as in new-born children. Inner border of feet raised more than normal, and with arch of feet exaggerated. No trace of equinus. Rotation of upper extremities complete. Elbows look backward and very slightly outward.
890	N. Y. H. M.	7 months	Rotation of legs complete. Tibiæ directed inward. Soles of feet look downward. Inner borders not abnormally raised.

later an equino-varus, position of the foot, which diminishes as the rotation of the lower extremity to its normal position progresses, although even when the rotation of the leg has been entirely completed, some of the varus deformity of the foot still remains; and a careful observer will notice a very slight tendency to varus even in the newly born child, which gradually disappears as the foot develops after birth. This fact is confirmed by Prof. Dalton. In addition to the varus deformity we often see in fœtuses of the second, third, and fourth months an equinus. This is not always present, and soon disappears in the course of the normal growth of the foot. We find, then, that at an early stage of fœtal life, talipes varus or equino-varus is physiological. Ordinarily, however, in a healthy fœtus little of the outward rotation of the leg or of the varus of the foot remains after the end of the sixth month.

Further study and observation will lead to the conviction that this physiological varus is dependent upon the primary outwardly rotated position of the leg in early fœtal life. To this conclusion we are led by even slight study of the relative positions of the thigh, leg, and foot during intra-uterine life.

As soon as the joints are formed, we find the thigh rotated outward as far as possible, and flexed upon the body. The leg is flexed upon the thigh, but not completely, for this is prevented by the extreme outward rotation of the thigh, which brings the inner border of the leg in apposition with the abdomen of the child. We have, then, the inner border of the thigh and the tibial border of the leg pressed against the abdomen of the fœtus, the legs crossing each other a little below their middle. (This position is typically shown in specimen 884 N. Y. H. M., described in the table). All of the intra-uterine pressure, therefore, is thus brought to bear directly upon the outer border of the thigh and leg,

corresponding to the fibular border of the leg, and also upon the dorsum of the foot. The result of this is that the foot is rotated in and extended (equino-varus) until the sole is almost on a line with the inner border of the leg, and lies against the body of the foetus, while the dorsal surface of the foot is on a convex curved line with the outer border of the leg, to adapt itself to the concave wall of the uterus. This, I believe, is a stage in the normal development of every healthy foetus; and were the extremities to remain in this position all children would be born club-footed. But nature provides against this by the inward rotation of the extremity, which gradually takes place, carrying the leg away from its position against the abdomen of the foetus; and when this rotation is completed, we find the extensor surface of the thigh flexed and in relation with the body of the child, while the legs are flexed upon the thighs, the inner or tibial borders facing each other. Now the soles of the feet lie against the uterine walls and the intra-uterine pressure is exerted directly upon them. This produces extreme flexion of the foot upon the leg, together with an outward rotation of the foot; this movement, from the constitution of the ankle-joint, accompanying extreme flexion. Thus is antagonized the varus or equino-varus which has existed hitherto. It is evident, then, that upon the completeness of the internal rotation or torsion which takes place in the lower extremity, depends the rectification of the early varus of the foot. Should this rotation not take place at all, or be incomplete, the foot will continue to maintain its early relation to the body of the foetus and uterine walls, and the child will be born more or less club-footed. If this be so, we should expect to find in club-footed children that the extremities are rotated outward. And this we do find upon examination. In all of the cases of congenital club-foot (equino-varus), which I have seen

since my attention has been directed to this subject, I have found, that the thigh and leg as a whole were rotated out, and the tibia bent inward at its lower part so that the feet were approximated to each other in addition to being in the clubbed position. All this is seen to be the result of the non-rotation of the leg. This can be confirmed by any one, upon a young child suffering from this deformity. At the Orthopædic Dispensary my colleagues have confirmed these observations.

Here I wish to call attention to the fact that no extraordinary amount of intra-uterine pressure is necessary to produce this deformity, its cause being not pressure, but as we have seen a non-rotation inward or a retarded rotation inward of the lower extremities. This I maintain in spite of the accidental occurrence of pressure-marks on the feet of a few club-footed children, and notwithstanding the observation, in an equally small number of cases, that the birth of a child with pes equino-varus was accompanied by a deficiency in the amount of liquor amnii. Both of these facts have been advanced as proofs that club-foot was due primarily to an abnormal amount of intra-uterine pressure by such observers as Volkmann,³ Luecke,⁴ Vogt,² Banga,⁶ and Kocher.⁶ Vogt himself acknowledges, however, that such an "*ad oculos*" demonstration of abnormal intra-uterine pressure is rare. On the other hand, almost every case of congenital equino-varus will show evidence of the condition to which we have ascribed the deformity: namely, non-rotation inward of the lower extremity. This is best measured by the direction in which the knee looks. In the large majority, and perhaps all, of the children with congenital equino-varus, the knee will be found looking outward, and only slightly forward.

Luecke⁴ states the direct opposite of this, and says that in these children the thigh is rotated inward, and the knees

form a genu valgus. That this statement is in contradiction to the actual position of the legs in congenitally club-footed children who have not yet walked, any one may convince himself by personal observation, and I can account for it in no way but that it was an unintentional error on the part of this writer.

In support of the theory, then, which I have here developed, we have :

1st. The fact that the leg is rotated outward in all cases born with this deformity.

2d. That during embryonic life a similar deformity is present, as long as the leg has not yet completed its inward rotation.

3d. That this view is perfectly compatible with the otherwise healthy condition of most of these children.

4th. That this theory readily harmonizes with the hereditary element in the history of talipes equino-varus congenitus. For it is easily conceivable that the same lack of developmental energy which characterizes one member of a family may be present in other individuals of the same family.

5th. That upon this basis can easily be explained some complicated cases of club-foot, which would otherwise appear as inexplicable anomalies: as, for instance, the occurrence of club-hand and club-foot in the same patient. Here the hand has undergone a retardation of development as well as the foot.

Adams quotes an extreme instance of this, which he does not attempt to explain. The case is that of a child with equino-varus of both feet; knee and hip joints stiff; radius turned inward and facing toward the head (this, as we have seen above, is the early fœtal position). The elbow and shoulder joints were in a condition similar to that of the knee and hip (page 221, *op. cit.*). Here, evidently, the outward

rotation of the upper and the inward rotation of the lower extremities have not taken place. Both have been kept in the primitive position—a position which does not admit of much motion. Torsion not taking place, even to a slight degree, the joints have never been exercised, hence their stiff and semi-anchylosed condition. Upon no other basis could such a concurrence of deformity in the upper and lower extremity have been so well explained.

In this connection I would describe a very interesting specimen in the Wood's Museum collection, numbered in the catalogue as 1220. "A fœtus showing general deformity." "Hands and feet clubbed." This is an eight-months' fœtus, in which the rotation of the lower as well as the upper extremities has not taken place. The result being that the extremities have retained the early fœtal position with regard to the body of the child; the legs are flexed at about right angles with the thighs, while the inner borders of the thighs, legs, and feet are pressed closely into apposition with the abdomen, thus causing an exaggerated equino-varus deformity of the feet, and stiffness of the knee, and especially of the hip joints, owing to the causes above stated.

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TWO CASES OF DIPHTHERITIC SPINAL ATAXIA.

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THE subject of paralysis consecutive to acute diseases of various nature has received ample notice at the hands of many writers on nervous affections. Especially have the paralytic derangements which follow diphtheria been accurately and thoroughly described ; it would therefore be entirely superfluous to reiterate what has been told so many times, and we shall consequently not enter into a description of diphtheritic paralysis in general, but refer more particularly to a certain peculiar form of these accidents.

The fact that the differential diagnosis between a diphtheritic paralysis of the lower extremities simulating locomotor ataxia, and a genuine case of tabes, may become difficult, seems to have attracted the attention of only a few writers upon this subject. In nearly all books the articles on locomotor ataxia, or those on diphtheritic paralysis, entirely ignore the possibility of the two diseases being mistaken for each other, and it is precisely on account of having met with this difficulty ourselves that we have been led to call attention to the cases which will be found further on.

As early as 1836 Orillard,¹ in Poitiers, mentioned ataxic symptoms as late sequelæ of diphtheria, and Trousseau, Sée, and Maingault had remarked the unsteady gait in certain cases of diphtheritic paralysis, and had even attributed it to muscular anæsthesia. Eiseman² and Jaccoud,³ however, were the first persons who seemed to separate the motor paralyse of the lower extremities following diphtheria from ordinary paralyse, and to classify them as cases of diphtheritic locomotor ataxia, and it is with them that the name diphtheritic ataxia originated. Eiseman and Jaccoud, however, do not agree, inasmuch as Eiseman considers all cases of anomaly of movement following diphtheria as ataxia, whereas Jaccoud makes a distinct differentiation between paralysis and ataxia. Afterward English writers, Grainger Stewart,⁴ Foot,⁵ and others, described cases of this disorder, and in late years several more have been added to the list, so that at present the existence of such a disease is undisputed. Eulenburg, who, in the first edition of his textbook, speaks of an "apparent incoördination," in his later editions distinctly acknowledges the existence of a real diphtheritic ataxia. Brenner⁶ speaks of diphtheria as being able to cause incoördination of movements without atrophy of the muscles, and with normal electrical contractility of both muscles and nerves. His is probably the best and most lucid classification of the nervous sequelæ of diphtheria.⁷ He divides the anomalies of movement following this disease into three groups. In the first group he classes those cases in which the interference of motion is due to pure ataxia. In the second, those in which it is due to an

¹ Maingault, *Arch. générales*, 1859, Oct., p. 388.

² "Die Bewegungs Ataxie," p. 192.

³ "Les paraplégies et l'ataxie du mouvement," p. 631.

⁴ *Edinburgh Med. Jour.*, 1870, p. 988.

⁵ *Dublin Journal of Med. Science*, 1872, p. 176.

⁶ "Untersuchungen auf dem Gebiet der Electrotherapie", ii, p. 207.

⁷ *Petersburger med. Zeitschrift*, Bd. x, Heft 4 and 5, 1866.

ataxic paralysis, characterized by a paresis of certain muscular groups, and the more complete paralysis of certain others. In the third, finally, those which are due to a true paralysis. The cases which come under the first heading, namely, those where the anomalies of motion are due to pure ataxia, are quite rare, and therefore those recorded further on will be of interest.

The facts about ataxia in general are so well known that it is not necessary to review them here; it is only requisite to state that we can divide all disassociation of movements into spinal and cerebral (includes cerebellar). We desire particularly to call attention to this point, for the cases of cerebral ataxia following diphtheria are not at all rare; in fact, in many cases of diphtheritic paralysis there exists a certain amount of ataxia, associated with obtuseness of intellect, dizziness, headache, and cerebral symptoms in general. These cases, then, have, by various authors, according to the preponderance of the paralysis or of the ataxia, been described as cases of diphtheritic paralysis or diphtheritic ataxia. Langner,¹ under the supervision of Prof. Berger, reports several cases of cerebral ataxia following diphtheria, and the majority of cases published as diphtheritic ataxia come under this category.

CASE I.—J. H., twenty years of age; grocer. Temperate habits; had always been well until the 17th of December, 1879, when he caught cold by exposing himself to the night air, and the next day complained of pain in his throat and difficulty in swallowing. These symptoms augmented in severity until the 22d, when he was obliged to take to his bed. He remained in bed until the 25th, during which time he had medical attendance. His physician told him he had "ulcerated sore throat," and that it was full of "blisters." He claims not to have had any fever. Remained in the house until the 2d of January, and then feeling quite well, and not having any more pain in his throat, he went to work again. Two weeks after he noticed that his voice had become

¹ Inaug. Dissert., Breslau, 1878.

nasal in character, that frequently while swallowing he spluttered the food out through his mouth and nose. He describes it as though a cramp of the muscles took place, which threw the food, especially fluids, back. At times particles of food also entered the larynx. These symptoms, however, only lasted a few days, and then disappeared. Thereupon he complained of weakness of the lower extremities, and following this, of the hands. The condition of patient when he came under our observation, March 1st, was as follows :

Patient is tall, strong-looking, and well built. Tongue, mouth, throat, and uvula normal. Swallows without any difficulty, has good appetite, and digests well. Circulatory, respiratory, and genito-urinary systems healthy.

Voice slightly nasal, but he claims that it was always so. In examining the upper extremities we found the sensibility to heat and pain normal. He does not localize perfectly, and there is a retardation in the appreciation of sensation of several seconds ; thus, when his eyes are closed, he does not feel the prick of a pin on the back of his hand until fully two seconds after he has been so pricked. The muscular power does not seem to be diminished, and the movements of his hands are not noticeably affected ; but the more delicate movements of the fingers, movements requiring great precision, are executed with unsteadiness. He cannot button his clothes, or pick up any small object. If he endeavors to pick up a needle from the table, he only succeeds in doing so after many awkward attempts. The lower extremities also retain full muscular power, but he walks very unsteadily and has a marked ataxic gait. His knees knock together at intervals while walking. The sensibility to touch, heat, and pain is normal, except in the soles of the feet. When he walks he says he cannot feel the floor distinctly, but that it feels as though he were walking on a sponge. The unsteadiness in walking is increased by closing his eyes. If he stands with his legs spread apart he is more steady, but when he brings them together, he must sustain himself to avoid falling.

Muscular development good.

Patellar tendon reflex entirely missing. Achilles reflex not obtainable. Reaction of both galvanic and faradic currents normal. All cerebral symptoms, as dizziness, headache, vomiting, anomalies of speech and intelligence, were wanting during the entire course of the disease. Patient was treated by sulphate of strychnine pills, $\frac{1}{60}$ grain each ; one pill

four times daily. Under this treatment he gradually recovered, until May 1, 1880, when, with the exception of the patellar reflex, which was still absent, he was entirely restored to his normal condition. In May, 1882, I again saw patient, and after this lapse of two years found the *patellar reflex present*, but weak. Otherwise patient is and has been perfectly well.

CASE 2.—H. R. came to consult us the beginning of June, 1881, and we then made the following notes in our case-book :

Family history is, with the exception of one living brother, who is idiotic, uninteresting.

Previous history: Patient was, with the exception of a short cough and occasional headache, both of which he has had since childhood, well until a short time ago. He served through the entire Franco-Prussian war, and was subjected to great hardships and deprivations. He, however, did not seem to have been affected by these, and remained in good health until last March, when he caught cold, had a sore throat, and remained in bed eight days. He says that his throat was covered with "white blisters." About fourteen days after getting out of bed, he noticed that his voice had become nasal in character; also, that he had difficulty in swallowing, especially so when he attempted to swallow liquids, as these were frequently thrown back through the nose, or, getting into the larynx, gave rise to violent spells of coughing. This condition lasted about three weeks, when he noticed quick-shooting pains in both arms and legs. These pains were very severe, and prevented him from obtaining any sleep. A short time thereafter, he began to feel unsteady on his legs, and the floor did not appear solid to him. He says it felt as though it were sinking from under him. He could hardly walk at all in the dark, and walking up stairs was very difficult at all times. Then his hands became affected, first formications, and then numbness of the tips of his fingers supervening. He did not feel objects that he held in his hand, but was cognizant of their presence only by watching them. Frequently let cups and saucers fall from his hands; this occurred particularly when he turned his eyes to something else. Had great difficulty in buttoning his clothes. These symptoms have increased in severity up to the present time.

St. Pr.—P. is a tall, well-built individual, of healthy appearance. Complains of sharp shooting pains in the extremities, but these he says are not as severe as they formerly were. Cannot stand with closed eyes, as he becomes dizzy and would fall if he

were not held. Has a feeling as though he were standing on a very thick carpet. Cannot walk in the dark. Muscular force of legs normal. His gait is decidedly jerky; lifting his foot high from the ground, he throws it violently forward, and then brings it sharply to the ground.

Patellar tendon reflex entirely absent.

The muscular force of the upper extremities is also undiminished. The muscles are not atrophied. Has a feeling of numbness in his fingers. With his eyes closed, he cannot distinguish between a pin and a penholder. He can distinctly feel the prick of a pin, but cannot localize it well. Cannot button his clothes without great trouble. Pupils react well. Eyes normal. Reaction to both electrical currents normal. The treatment employed was: sulphate of strychnine, three times daily, $\frac{1}{60}$ of a grain, internally; and, three times a week, a descending galvanic current of sixteen Léclanché elements, along the spinal cord.

August 1st. Patient is somewhat improved. He still cannot walk in the dark. There is a decided improvement in the hands, as he can now button his clothes and dress himself unaided. He claims that they still, however, feel somewhat numb. Patellar tendon reflex still absent.

I then lost sight of patient, and did not again see him until January, 1882, when I had opportunity to re-examine him, and found him fully recovered; able to walk without any trouble. No apparent ataxia, either of hands or feet. *Patellar tendon reflex normal.* Says he never felt better, and that he can walk long distances, and is again able to work at his trade, which, being that of watchmaker, requires fine and precise work.

In these cases, therefore, there being only an incomplete history of diphtheria, and the patients coming under observation after all diphtheritic symptoms had passed away, and at a period when none but tabetic symptoms were present, the possibility of mistaking the disease may be appreciated; the second case particularly, as it occurred in a man who had committed excesses of various kinds, and been exposed to manifold deprivations. There can be no doubt whatever as to the existence of the ataxia, the unsteady gait, the planting down of the heels, the throwing, jerky movements in walking, the unsteady and irregular

movements of the hands, are probably sufficient as proofs of its existence. The only question yet remaining which admits of any doubt, is whether the ataxia in these cases can be termed identical with that existing in *tabes dorsalis*. Certainly, considering the absence of all symptoms which are generally present in cerebral ataxia, we are not warranted in searching anywhere outside of the spinal cord for an explanation of this incoördination.

Dizziness, headache, vomiting, anomalies of speech, or cloudiness of the intellect were not present during the entire course of the disease. On the other hand, the symptoms which were present, such as the formications, the dizziness when standing with closed eyes, the gradual course, first the lower and then the upper extremities becoming affected, and, above all, the anomalies of reflex action, the absence of patellar reflex, render it almost a certainty that the anatomical site of the lesion does not differ from that found in progressive locomotor ataxia, although, considering the course of the disease, and the rapid convalescence, we certainly cannot think of a sclerosis, but must attribute it to some more ephemeral lesion, if we have the right to apply the word lesion at all.

A very important feature of these cases, and one to which very little attention has been paid in cases of diphtheritic ataxia, is the absence of patellar tendon reflex, with its subsequent return. Rumpff,¹ in reporting a case of pure spinal diphtheritic ataxia, of which I will mention the salient points, speaks particularly of the absence of patellar reflex. So also does Langner.² Rumpff's patient was a girl, nine years of age, who had a severe attack of diphtheria, and who, during the course of the disease, had difficulty in speaking and swallowing. Four weeks subsequently, pare-

¹ *Deutsches Arch. für klin. Med.*, 1877, vol. xx.

² *Inaug. Dissert.*, Breslau, 1878.

sis of accommodation, insufficiency of the recti interni, and paralysis of the soft palate. After fourteen days she became easily fatigued, had a staggering, unsteady gait, and the more minute movements of the fingers were hampered and executed with difficulty. Reflex of patellar tendons absent. Weakness of extremities. After another lapse of fourteen days, the heels are brought down suddenly, the feet thrown forward, and the skin reflexes almost absent. Muscular force normal. Ataxia of lower as well as upper extremities. Gradual improvement, and after four months power of coördination was again present. The tendon reflexes, however, remain absent for five more weeks.

Autopsies have not been frequent in diphtheritic paralysis on account of the rarity of a lethal issue during this condition, and formerly the result of the few *post-mortems* which were made was always a negative one. This, however, need not surprise us when we consider that the aid of the microscope was rarely called into requisition, and that when it was, it was done without the use of the staining materials which are now at our command, and which are of so much service to us in histological and pathological research.

The supposition of Bretonneau, that the paralysis is only a local extension of the diphtheria, is to be cited solely for its historical interest.

Trousseau's theory, which was also supported by Maingault and Sée, that the paralysis is due to systemic poisoning, the blood becoming vitiated by the absorption of the diphtheritic virus, becomes improper to sustain normal nutrition and the normal excitation of the nervous centres, *ergo* the paralysis, certainly at first makes a favorable impression upon the mind; it has now, however, become entirely untenable. This theory of poisoning has been strongly contested by Prof. Gubler, who observes that

sometimes cases of ordinary sore throat, without any specific character whatever, are followed by paralysis of the soft palate. The simple œdema which occurs in most all of these cases would, however, be sufficient to account for this local paralysis. Whether the paralysis be due to vitiation of the blood or not, the question which will naturally propound itself to us, is whether there is any nervous lesion, and if so, where and what that lesion is.

This can be best answered by briefly reviewing the facts now known. The present state of our knowledge, according to Dr. Magne, is as follows :

In 1862 Charcot and Vulpian in a woman deceased during a diphtheritic paralysis of the soft palate, found evident changes in the motor nerves supplying this part. In 1869, Lorrain and Lépine mentioned a similar case, and Liouville has observed an analogous change in the phrenic in a case of diphtheritic paralysis of the diaphragm. Bühl, in 1867, noticed a marked alteration in the roots of the spinal nerves, the cord itself being unaffected. He found the spinal roots and ganglia thickened, discolored, and softened; the neurilemma and the interstitial substance diphtheritically infiltrated. His theory is that the paralysis is caused by this exudation, and if it is absorbed the case ends in recovery, but if, on the contrary, it is changed into connective tissue, the normal state is not restored. Bühl also found capillary hemorrhages in the brain.

R. Maier, in 1870, found microscopical changes which corroborate those seen by Bühl.

Oertel has noticed capillary hemorrhages in the rachidean as well as in the cerebral dura mater; also in the sheaths of the nerve-roots and of the peripheral nerves. In the anterior cornua of the cord, he observed an increase of nuclei and small hemorrhagic infarctions. He also found larger hemorrhages in the brain, cord, and peripheral

nerves, and found the central canal occluded by a cellular exudation.

H. Weber¹ supports the view that the paralyses originate by means of a change which is unknown to us, and which is as yet undiscoverable, starting at the periphery of the nervous system (the seat of the diphtheria) and being propagated to the spinal cord in an analogous manner, as in traumatic tetanus, the propagation takes place from the seat of injury.

Pierret² in examining the hardened spinal cord of a man deceased during a diphtheritic paralysis, which in the fresh condition was of normal appearance, found a number of changes. The dura mater was adherent, partly thickened and roughened. Upon section the diphtheritic changes became apparent. He found a meningitis, and an exudation in the meninges similar in all points to the pseudo-membranes in the larynx. Vulpian, however, who examined three other cases subsequently, failed to corroborate this.

A more recent investigator of the subject is Déjérine,³ who in five cases found the same kind of lesion. He found in all his cases a parenchymatous degeneration of the anterior roots. He found constant light changes in the gray substance of the anterior horns. He considers the whole as a slight parenchymatous and interstitial polio-myelitis anterior, and looks upon this as the primary change in diphtheritic paralysis. The alterations in the nerves and in the muscles are, according to his view, only secondary trophic changes. The various sites of the changes, then, as they have been found in the divers cases, would also explain the diversity of the paralytic symptoms. Thus, to explain

¹ *Virchow's Archiv*, xxv.

² *Gazette des hôpitaux*, No. 2, 1877.

³ Déjérine : *Recherches sur les lésions du système nerveux dans la par. diph.* *Arch. de physiol.*, 1878, p. 107.

the paralysis of the soft palate, the tongue, and lips, we must place the seat of lesion in the medulla oblongata. So again in cases where there were anomalies of sight and hearing accompanying the paralysis, the origin of the optic and auditory nerves were affected. In a case where Pierret found the changes in the lumbar region, the subject had been paraplegic. We have not been able to find any reported autopsy of a case of pure diphtheritic ataxia, therefore are unable to say whether the seat of lesion is in the posterior columns of the cord, as from our cases with their tabetic symptomatology and their long ensuing absence of patellar reflex, we would be led to suspect.

As regards the prognosis and treatment there does not remain very much to be said. Most cases end in recovery, and are not of very protracted duration. The treatment employed in all reported cases has been the use of the sulphate of strychnine, internally or hypodermically, the use of the galvanic current along the spine; or both of these measures combined. Whether the disease is curtailed in its course by these measures, or whether it is self-limited, would be hazardous to say, as the cases from which we are forced to draw our conclusions are not sufficiently numerous.

NOTE ON CRANIO-CEREBRAL TOPOGRAPHY. ILLUSTRATED.*

By E. C. SEGUIN, M.D.

THE surgical anatomy of the head, with reference to its contents, has been developed with remarkable completeness within the last ten years, chiefly by the researches of Broca, Bischoff, Heftler, Turner, and Féré, by which cranio-cerebral topography has been firmly established as a branch of practical anatomy. Acting upon the data thus obtained, Broca, Lucas-Championnière, Weir, and others have successfully trephined for the relief of aphasic and paralytic symptoms. The location of many convolutions and fissures of the cerebrum can be accurately mapped out upon the surface of the skull, or even upon a living head, by the projection of certain lines and measurements from certain points thus obtained, as well as from some natural landmarks.

For the projection of these lines, the head is placed in a particular position, as can be easily done when we operate upon a bare skull; but which can also be approximated when we deal with a living subject, either sitting up or lying in bed. The skull or the shaven head should be so placed and held that a line passing from the alveolar process of the superior maxilla just at the insertion of the teeth, and through the lowest part of the occipital bone, shall be

* Reprinted from Gross' "System of Surgery."

external auditory meatus. At the top of the head this line—the auriculo-bregmatic line,—A-A, indicates the bregma or true vertex, which is an important landmark, one which should be traced with ink or carmine upon the shaven scalp. Upon the top of the head or skull an imaginary horizontal line, 4-4, parallel with the alveolo-condyloid plane, is projected, and upon this line, 4-4, we measure backward 50 mm. and draw a second vertical line, B-B, parallel to the auriculo-bregmatic line. At the place where this line strikes the convexity of the head is the Rolandic point, R, under which, in average heads, lie the upper or posterior extremity of the fissure of Rolando, the upper ends of the ascending frontal and ascending parietal convolutions, and, within the longitudinal fissure, the paracentral lobule; in other words, the upper part of the motor area of the cerebrum, that which quite probably controls the volitional movements of the legs. The Rolandic point, thus determined, should, on a living subject for operative purposes, be properly marked upon the shaven scalp.

A third horizontal line is next to be drawn from the external angular process of the frontal bone, parallel with the alveolo-condyloid plane. This line, 2-2, which may be called the fronto-lambdoidal, as its posterior extremity will usually pass at or near the upper angle of the lambdoidal suture, serves the purpose of determining the situation of some important parts. In the first place the line 2-2 passes about 5 mm. above the upper border of the squamous suture, and under this line, mostly parallel to it, are the anterior two thirds of the fissure of Sylvius. Secondly, at about 5 mm. above and behind the intersection of lines A-A and 2-2 is the inferior extremity of the fissure of Rolando bounded by the ascending frontal and ascending parietal convolutions. In the third place, upon this line, 2-2, at a distance of 18 or 20 mm. behind the external an-

gular process of the frontal bone, is the folded part of the posterior extremity of the third frontal convolution, or Broca's speech-centre, marked F³ on the diagram.

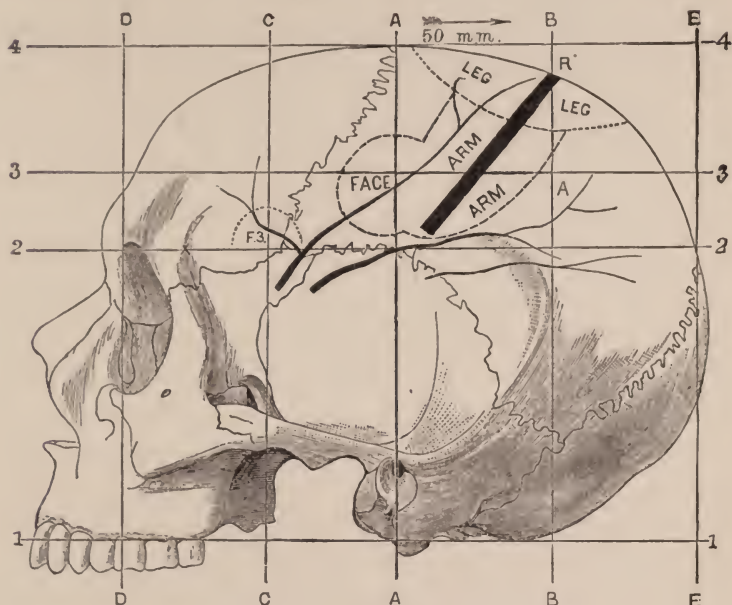


FIG. 2.

Topographical lines applied to Henle's skull. Location of the Rolandic fissure, motor centres, and branches of the middle meningeal artery shown.

Having exactly determined and marked the situation of the Rolandic point at the top of the skull, and the inferior termination of the fissure of Rolando above the ear, these two points are connected by a line which is represented darkly drawn upon the diagram. This, the Rolandic line, is the guide for nearly all operations intended for the relief of traumatic spasm or paralysis, since under it and near it lie the so-called motor centres for different parts of the body on the opposite side, as determined by experiments upon monkeys and dogs, and by numerous post-mortem examinations made in cases of tumors and other limited lesions of the brain.

As indicated by the dotted lines on the diagram, the motor zone or centre for the lower extremity of the opposite side lies about the Rolandic point, making an allowance of at least 10 mm. to either side of the median line for the interval between the two hemispheres. It also includes the paracentral lobule within the longitudinal fissure; and Dr Seguin is led to believe, from observation in cases of cerebral tumor, that this part is preëminently the centre for the leg. Below this, reaching quite down to the fronto-lambdoidal line 2-2, is the motor area of the upper extremity. Forward of this, between the auriculo-bregmatic line A-A and the line C-C, is a part of the second frontal convolution, which probably has connections with the facial muscles of the opposite side. Finally, at F^s is the speech-centre of Broca, which, although not now regarded as the only speech-organ, yet plays an important part in the simpler mechanism which produces language-motions.

Other relations of interest are the apex of the sphenoidal or temporal lobe a little beneath the line 2-2, and at about 10 or 12 mm. posterior to the external angular process of the frontal bone; the situation of the occipito-parietal fissure almost immediately under the posterior end of the line 2-2, at its junction with the vertical line E-E, which also indicates the posterior extremity of the cerebrum; the anterior end of the brain is marked off by the vertical line D-D. Furthermore, for certain purposes, the limits of the central ganglia of the brain may be estimated as follows: their superior limit is indicated by a horizontal line or plane drawn at a level 45 mm. below the vertex, line 3-3 of the diagram; their anterior limit, which corresponds to the head of the nucleus caudatus, is traced by the vertical line C-C; and their posterior limit, or hinder end of the thalamus opticus, by the vertical line B-B. Lastly, it may be stated that the angular gyrus—a part of the cortex which

recent autopsies would seem to connect with vision—lies not far from the point of intersection of the lines B-B and 3-3, at the point marked A on the diagram. This, in the living subject, is a little below and behind the parietal eminence.

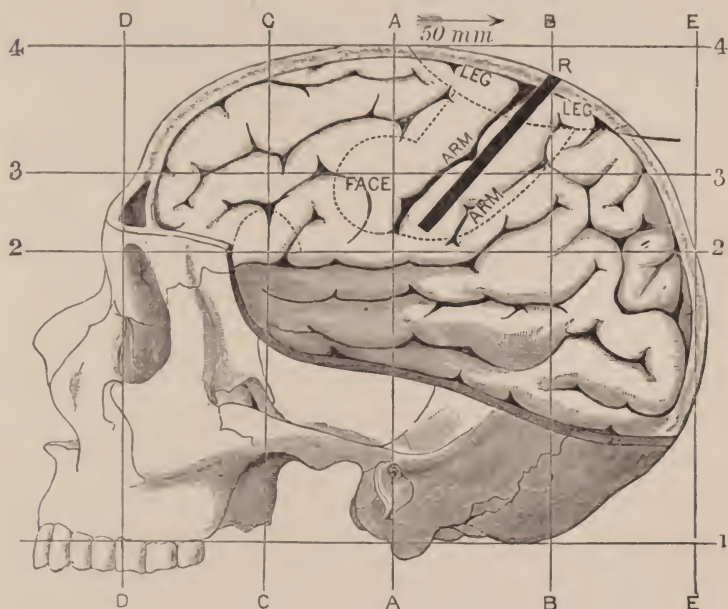


FIG. 3.

Topographical lines and landmarks projected on the convolutions, the apparent non-conformity of the Rolandic line and fissure being due to perspective. Henle's skull.

The location of the middle meningeal artery, which so often furnishes the blood which compresses the brain after various injuries of the head, is surgically considered of great importance. The course of the two principal branches of the artery is approximately indicated upon the diagram by the branching lines drawn on the anterior inferior angle of the parietal bone. In the living subject, the main trunk of the artery would be found under the horizontal line 2-2, at a point a little posterior to the speech-centre, about 30 mm.

behind the external angular process of the frontal bone, and in front of the beginning of the fissure of Sylvius. It passes obliquely upward and backward almost immediately over the whole of the ascending frontal convolution, from 5 to 10 mm. in front of the Rolandic line. The inferior branch of the artery is nearly horizontal, and almost exactly overlies the fissure of Sylvius.

Upon the shaven head of a patient, seated in a chair or lying in bed, the principal landmarks and relations above defined can be mapped out with a great approximation to accuracy by the use of two rulers, or even by one, to mark the alveolo-condyloid plane, and a card-board cut so as to stand astride the skull in the auriculo-bregmatic vertical. A light wooden apparatus could be easily made to indicate these two lines, while the remaining measurements could be taken with a tape, and the points marked with carmine or black ink.

The practical utility of these anatomical data depends upon an acceptance of modern physiological teaching upon the subject of the functions of the brain. The experimental and pathological evidence now accumulated in favor of the connections of the "centres" marked on the diagram and certain peripheral parts, and between the whole of the motor area, and the whole opposite side of the patient is, according to Dr. Seguin, convincing, and leads him frequently to a very accurate topographical diagnosis in medical cases.

The following operations may be referred to as illustrative of the utility of the laws of cerebral localization and of cranio-cerebral pathology :

Broca, in 1871, in a case in which aphasia and paralysis followed a severe lacerated scalp wound, trephined over the left third frontal convolution, or speech-centre, found pus, and slightly relieved his patient.

Lucas-Championnière, in 1874, trephined a man in whom coma, partial right hemiplegia, convulsions, and, as shown during convalescence, aphasia resulted immediately from a slight cranial injury. There was only a slight scalp scar to guide him, but he came in contact with splinters and blood from a fracture existing below the point of apparent injury, and saved his patient.

Hueter, of Greifswald, in 1879, in a somewhat similar case, trephined the skull, ligated the middle meningeal artery, and cured his patient. In another case in 1870 he was equally successful.

Courvoisier trephined, in 1878, a child two and a half years old, who, after an insignificant wound in the left temporal region, had right hemiplegia, coma, and palsy of the left side of the face. He found a fissured fracture, and pus outside the dura mater, as well as a large quantity under it. The operation was followed by recovery with weakness of the right side.

Dr. R. F. Weir, of New York, in 1882, operated at Bellevue Hospital, in a case in which coma and slight hemiplegia existed, the patient being a man who had received a blow on the head. There was no very evident external injury, but guided by the various data of cerebral localization, and proceeding according to the rules of cranio-cerebral topography, the trephine was applied, and a small clot found between the brain and dura mater. On incising the latter the brain was seen to be extensively disorganized, and the seat of copious hemorrhage, which was checked by torsion. Although the symptoms were relieved by the operation, death occurred within a few days.

Up to the present time, so far as I know, there are only three cases in which the rules of cranio-cerebral topography have been applied, from measurements actually made prior to operation. These cases occurred in the practice, respec-

tively, of Broca, Lucas-Championnière, and Weir. There are probably other examples, but I am unable to particularize them. However this may be, there is a large number of recorded instances of relatively or absolutely successful operations performed after cranial injuries, for immediate as well as for secondary effects, without measurements.

The indications for trephining after cranial injuries for the relief of symptoms of cerebral irritation, compression, or disorganization, may be provisionally stated as follows:

1. When aphasia supervenes immediately or within a few days or weeks after an injury of the anterior portion of the head on the left side. It is extremely probable, in the first case, that a clot or bony spicule will be found compressing the speech-centre; and in the second, that an abscess has formed either in the same part or close to it.

2. When simple hemiplegia, or hemiplegia with hemispasm, follows an injury, however slight, in the temporo-parietal region. If the spasm or paralysis be limited to one limb or to the face, the indication to operate is even stronger. Even if the injury be not directly over the motor area, the surgeon is justified in such a case in exploring that area.

3. In conditions of coma after cranial injuries, sometimes without external wound, in which meningeal hemorrhage is the cause of death, the discovery of slight hemiplegia should call for trephining planned according to the rules above laid down, as in Weir's case. Dr. Seguin suggests that a latent hemiplegic state might be discovered, at least in some cases, by an increase of peripheral temperature, as of the fingers or toes, on one side, and by the presence of congestion or of an erythematous blush on one buttock.

4. In the very rare cases in which the paralytic phenomena are found on the same side of the body as the cranial injury, it might be proper to trephine on the opposite side

of the skull in search of hemorrhage or fracture, the result of contre-coup.

5. In chronic epilepsy after traumatism of the head, the indication for operation is present, but it is not a specific indication, connected with the subject under consideration. A lesion of any part of the skull may be a cause of epileptic attacks, irrespective of the motor centres.

Contra-indications to trephining may be thus enumerated :

1. Whenever in apparently favorable cases there are signs indicative of lesion of the base of the brain, such as palsy of several cranial nerves, neuro-retinitis, or Cheyne-Stokes respiration.

2. When hemiplegia is accompanied by great anæsthesia, rendering it probable that the lesion is beyond the motor area, deeper, and farther back.

It should be understood that these indications and contra-indications are formulated from the stand-point of the neurologist. The surgeon, upon general grounds, will doubtless often modify them, and add others.

It is obvious that our knowledge of cranio-cerebral topography is not perfect, and, in a practical point of view, very unsatisfactory. As yet, we have broken only a little ground ; the great work remains to be accomplished. In a field so vast as this our progress must necessarily be tardy, and many hands will be required to solve its mysteries.

EDITORIAL DEPARTMENT.

HOSPITAL MANAGEMENT.

It gives us great pleasure to bring to the notice of the general medical public the fact, that in two of our best hospitals steps have been taken toward lengthening the terms of service of the attending staff.

Attention has been repeatedly called in the editorial columns of this journal to the existing evils in hospital management in America, and the almost universal prevalence of short terms of service has been justly deprecated.

Public hospitals serve, or should serve, two ends, both of which are for the public good. The first is, as humanitarian institutions, the welfare of their helpless inmates ; and the second is, the advancement of the science of medicine.

To show how a lengthened term of service will conduce to these two ends, which are really one, a brief review of facts is necessary.

In most of our city hospitals a division is made into a medical and a surgical side or service. In some a subdivision of these services is made. Each one of these services, consisting of from fifty to one hundred patients, is under the superintendence of several attending physicians or surgeons, who in rotation assume charge of the entire service. As they are generally four or more in number, and as each generally wants to be on duty once every year, it reduces their term of service to three months or less.

In the first place, the attending has more patients than he can do justice to. When he comes on duty he finds a great many

cases convalescent through his predecessor's skill. These cases possess little to interest him. Another lot of cases are those upon which three months' treatment will make but little impression, and these, too, very naturally fail to interest him. The third set of cases are those which enter the hospital during his service, are afflicted with acute diseases, which, whether under his treatment they tend to speedy cure or dissolution, never fail to excite his unflagging interest, and to receive his almost undivided attention. What is true of one attending's actions is true of all. The convalescents, left by one, get well as best they may. The acute cases receive his whole attention, while the chronic cases are passed on from one to another, and from one hospital to another, receiving nowhere the attention they need. What is true of the attendings is true also of the internes. They have too much to do. The mere fact of their having more than they can do well prevents their doing as much as they otherwise might. Following the example of their elders, they devote most of their time to the care of the acute cases, to the exclusion of the study and care of those chronic cases which in later years will form the bulk of their practice.

The existence of these facts, which no man who has had a hospital experience will deny, proves conclusively the welfare of the patient is not subserved by short terms of service and inadequate house-staffs. Neither is the hospital as an educational institution bettered by this state of affairs. The learners in a hospital are first the attendings, next the internes, and lastly the general medical public. The advantage to the attending of a six months' over a three months' service need hardly be mentioned. The service being presumably subdivided, he can spend more time in the study and care of individual cases. He can watch the more protracted cases from beginning to end. He can teach his internes methods of investigating, recording, diagnosing, and treating cases, now impossible. He can avail himself and make available to others countless clinical facts by bedside and lecture-room talk. He can present to the general medical public through journal articles and monographs the result of his experience or

original investigations. The staff of internes will necessarily be doubled. This will give so many more young men a hospital experience, which is so essential to future success. They cannot plead overwork as an excuse for dereliction of duty ; better discipline will be maintained, and they will have time for outside reading and study.

It thus seems that a careful consideration of the subject could lead only to the conclusion that a lengthened term of service would result only in good.

The step taken by the attending physicians and surgeons of St. Luke's Hospital, where a six months' service has already been inaugurated, and a doubling of the house-staff asked for, and by the surgeons of the New York Hospital, where a similar change has been asked of the governors, deserves the heartiest commendation of all. Whether the suggestions of the respective medical boards will be carried out by the governing boards, remains to be seen.

NEW BOOKS AND INSTRUMENTS.

The International Encyclopædia of Surgery. A systematic treatise on the theory and practice of surgery by authors of various nations. Edited by JOHN ASHHURST, Jr., M.D., Professor of Clinical Surgery in the University of Pennsylvania. In six volumes. Vol. 1. New York : Wm. Wood & Co., 1881.

Anybody familiar with the eminent success achieved by Billroth and Pitha's collection of monographs on surgical subjects, later on of that of Ziemssen's, and finally of Billroth and Lücke's encyclopædias, will find it very natural that the embodiment of a similar idea in the English tongue was a mere question of time. Careful study of certain parts of this eminently American work—its international title to the contrary notwithstanding—will well repay the trouble. At the head of the first volume appears the well-rendered translation of a treatise by Professor S. Stricker, of Vienna, on "*Disturbances of Nutrition ; the Pathology of Inflammation.*" Though some enthusiastic adherents of Professor Stricker's peculiar views on many subjects of normal and morbid pathology have popularized his name to a certain extent on this side of the ocean, yet it will appear odd to many that an undoubtedly indefatigable and original investigator, but one whose views are still opposed to a remarkable extent by the majority of equally earnest workers among his countrymen, should have been selected as the exponent—so to say—of German histological science. The pleasure of reading the lucid exposition of the thoroughly original views of Prof. Stricker will certainly be appreciated by every practitioner familiar with the other side of the many controverted questions, and far may it be from us to forget that many of the novel aspects of old questions as set forth are full of suggestions and possibly fruitful promises. But it is difficult to evade the thought, that the practising surgeon will rather look in a similar

work for a concise and impartial exposition of the state of histological knowledge of the day, than for an undeniably able and very interesting but thoroughly *ex-parte* statement of strongly contested and hitherto uncorroborated theories.

On the other hand the selection doubtlessly has points that will commend it. The lack of original investigators among the large number of intelligent men in the American profession has been repeatedly commented on publicly, in journals and otherwise ; and to illustrate the attractiveness of the intimate and subtle machinery of the mental work of an original student, the choice could hardly have fallen on a more interesting personage than Prof. Stricker. Moreover, the tendency of modern science has clearly demonstrated that the object of scientific teaching is not so much the imparting of the simple knowledge of facts, but rather the training of the student's mind and senses in the methods of ascertaining by his own effort, by search, observation, and criticism, the facts included in any special branch of knowledge. And Stricker's treatise certainly possesses many of the features of this very modern tendency. It reads almost like a pleasant story, and its lucid style, the new light thrown on many still dark subjects, bits of historical reference and controversy, add much to the attractiveness of the otherwise dry subject. It would be out of place to enter here on a detailed criticism of the several views held by the author, and it will suffice to mention some leading features of the treatise, together with a few glimpses on such views that are novel and peculiar.

The treatment of the difficult subject of the vaso-motor nerves is excellent, and will be read with pleasure by everybody. The difference between the two forms of active hyperæmia, that of *paralysis* and that of *irritation*, together with the rationale of their vaso-motor influences, namely paralysis of the constrictors on one side and irritation of the vaso-dilators on the other, is well elucidated. The confirmatory vivisectional experiments are extremely interesting. The views held by the author about *internal flowing* and *undulating motion* of the cell-body ; the relations of cell and basement- or intercellular substance, and their interchangeability under certain circumstances, which is sometimes pressed to absolute identity of both, are very attractively exposed. Of the four cardinal symptoms of inflammation, two, viz.: heat and redness are contracted into one conception, that of active hyperæmia. Pain and swelling, not being necessarily and universally met with in inflammation, are discredited as trustworthy symptoms of the

process ; and *tissue metamorphosis* is insisted on as being of cardinal importance. The so-called migration theory of suppuration, first brought forward by Cohnheim, is declared as not proven, and in its place the author's own theory of tissue metamorphosis is proposed, with the arguments pertaining thereto. *In nuce* the theory is as follows : Under certain irritative influences the basis substance is converted into cell substance ; the cells return to the embryonic state ; then they divide into ambœoid, movable masses, which become detached from each other and thus acquire the character of pus cells. This theory holds good for all kinds of tissue, and the attempt of applying it to tissues of such divergent nature as the cornea, cartilage, bone, nerve substance, muscle, and connective tissue is carried through with much ingenuity and consistency. The fixed nature of the cells of the cornea, bone, cartilage, and other tissues is seriously questioned ; their essential identity and interchangeableness is vigorously insisted upon, and only one metamorphosis of the living substance is admitted as resulting in a permanent form of tissue, which, however, is devoid of functional capacity, and that is the so-called *fibrillar degeneration* as exemplified in *tabes dorsalis*.

The theory possessing the undeniable advantage of simplicity, is easily made use of to explain the process of healing by first intention and by suppuration, likewise that of ulceration, and the regeneration of muscular and nerve tissue.

The prevalent systems of classification of pseudoplasms are brushed aside as *mere child's play*, and unworthy of serious consideration, but no substitute is suggested therefor.

As previously mentioned, the translation by Dr. A. Meyer is very good and remarkably free from Germanisms.

Prof. Van Buren's essay on "*Inflammation*" implies not only the necessity of looking at the more practical or clinical aspects of this process, but can, in a manner, also be accepted as an apology for the one-sidedness of the first paper of the series. Otherwise it would appear to be superfluous to go again over ground presumably covered by Stricker's article. In the theoretical, introductory parts of the paper due attention is paid to the views of other pathologists than the Viennese professor, and the desire for impartiality and thoroughness as regards the enumeration of all possible causes and *modus operandi* of inflammation is very apparent. The term "enumeration" is purposely chosen, so far as the arrangement of the matter is concerned.

The leading theories of modern investigators, such as Pasteur, Koch, Ranvier, and others, are noted up to the date of publication. The juxtaposition of the latest information regarding septicæmia with notions belonging to an obsolete period of pathology, is striking. Cold and sudden chilling are mentioned as direct causes of inflammation, though the actual proof falls short of conviction. The illustrative case given with reference to this point is an instance of *post hoc ergo propter hoc*.

Among the mass of useful knowledge, statements occur which must challenge doubt. So it is said that the puerperal state, as such, predisposes to suppuration, and that collections of pus in joints *post partum* lack a satisfactory explanation. The etymology of the Greek affix, "*itis*," used to denote inflammation, is far from being as simple as given by Van Buren. That the uniform high temperature of injured organs, as in subcutaneous fractures or tenotomy wounds, plays any essential part in determining a kind process of healing, is, to say the least, very problematic; and that even a moderate addition to the habitual use of alcoholic beverages predisposes to suppurative inflammation, is simply not proven. That the practice of inoculating the cornea and conjunctiva with fresh pus to cure pannus is employed *on a large scale* in Germany, will strike most German oculists as a startling statement.

The favorable results after operations for disease, contrasting so markedly with the higher proportion of deaths after primary amputations, are explained by a sort of adaptation to, or tolerance of, operations, in consequence "of a previous training by illness and pain." In contradiction to this, Oberst¹ proved, from the extensive experience of the surgical clinic of Halle, that many deaths after primary amputation are due, on one hand, to the shock and loss of blood sustained with severe injuries, necessitating immediate operation; on the other hand, to the fact, that in primary amputations the larger size of the cut surface of generally robust limbs, the quantity of severed vessels, etc., render the successful practice of antiseptic surgery much more difficult than in the smaller transverse sections of the usually emaciated members of patients undergoing secondary amputation. In short, the danger from amputation can be said to increase in a direct ratio with the size of the cut surface.

The author is much more happy in the treatment of the purely practical portion of his essay, and here it becomes evident that the

¹ Die Amputationen unter dem Einfluss der antiseptischen Behandlung, Halle, 1882.

careful and observant surgeon is treading on ground well explored by himself. It is gratifying to read the sound and judicious remarks of Prof. Van Buren about the use or abuse of poultices, so commonly met with ; on local and general blood-letting ; on the practice of incisions and drainage, and nursing in general.

Prof. Alfred Stille's paper on "*Erysipelas*" is a fair model of a terse, condensed, yet withal attractive representation of a subject full of difficulties. Throughout it bears the marks of a thorough knowledge and assimilation of the literature of the matter in hand, and in drawing conclusions the author displays the clear unbiassed judgment of a philosophical observer. Arrangement of matter and style exhibit the skill of an experienced and successful teacher. Signs of careful preparation are met with everywhere. Conclusions are drawn deliberately, sometimes boldly ; and a refreshing critical spirit, untrammelled by the traditions of the shop and worthy of a philosopher, pervades the sentences. The remarks on the general treatment of the malady are replete with good-sense, and form a fit rebuke of the popular chase after specifics, the use of which is only too often the screen to hide mental indolence or incapacity, or both. Among the generally sound and unapproachable statements, exception has to be taken to some. The idea that the ancient treatment of erysipelas by applications of cold water is dangerous, lacks objective support, and is smacking of prejudice. The assertion that *alcoholic stimulants increase the fever* is flatly contradicted by recent observers. The preoccupation in favor of the tincture of chloride of iron, and the detractory opinion held about quinine, stand in strange contrast to the general tone of the essay. The author's above-mentioned objection to alcohol, when made by others as to iron, is thus disposed of : "The question is not whether iron raises the temperature, but whether this preparation of it tends to cure erysipelas." That equal success is claimed for quinine, whether administered in small or very large doses, is mentioned as a fact tending to discredit the assertions of its adherents. But Prof. Stillé forgets that this circumstance holds good as to the tincture of iron also. We do not find any mention of the author's own experience with quinine in erysipelas, and therefore conclude not to take his remarks, "*au sérieux*."

In the perusal of this paper time will be well employed.

Dr. Francis Delafield's well-known name is a guaranty of good work, and the wisdom of the author in confining himself to

a brief, but pregnant and clear summary of the debated questions pertaining to "*Pyæmia and Allied Conditions*," is very commendable. The embodiment of this conception will best satisfy the demands of the active practitioner for whom this encyclopædia has been admittedly gotten up. The indubitable facts bearing on pyæmia are well and clearly stated; the doubtful points are characterized as such; and the decision is left to a jury, composed of all minds interested in unravelling the mysteries of this most terrible enemy of the practical surgeon.

That condensation and terseness may be carried too far, Dr. Wm. S. Forbes' paper on "*Hydrophobia and Rabies; Glanders and Malignant Pustule*" fairly illustrates. A book of reference ought to present more of the pathology of a well-known morbid process like malignant pustule, than a mere sketch of a sketch. Davaine's discoveries are so well authenticated, and having formed the historical starting-point for myco-pathological researches now occupying the attention of the entire scientific world, would seem to deserve more than six lines of trivial statement.

Henry Trentham Butlin, the author of the disquisition on "*Scrofula and Tubercle*," seems to enter into his subject under the shadow of an unnecessarily gloomy view respecting the state of our knowledge of its pathology. For after providing a palpable substratum to his deductions in the shape of a few well-chosen histories of cases, he manages to extract from the chaos of contemporaneous information a tolerably correct picture of the anatomical and clinical features of both conditions. And since the proper combination of clinical observation with the pathology of disease will in all probability forever remain the only safe and popular way of studying maladies, the author can be said to have tolerably well fulfilled the requirements of his task. Recent investigation is noticed up to the date of publication, and the new aspects of tubercular pathology are set forth in a manner to suggest more exhaustive study.

"*Rachitis*" found a very able exponent in Dr. J. Lewis Smith, of New York, whose long-extended experience in this special field adds much weight to his opinions on practical questions of treatment of this malady. General and experimental pathology are adequately considered, and although the manner is not brilliant, the matter is well selected and perspicuously arranged.

Whatever Surgeon-General Wales may have to say on "*Scurvy*" will be accepted by the profession as coming, "ex cathedra." The editor of the *Encyclopædia* must be congratulated on the happy choice of this author for this particular theme. An erudite and yet attractive historical introduction at once captivates the interest of the reader ; and the careful arrangement of the quality and quantity of matter, together with the elegance of literary style, co-operate in forming a monograph, complete and well rounded off, and certain to bring credit to both the author and the entire publication.

Professor Verneuil's favorite theme, "*The Reciprocal Effects of Constitutional Conditions and Injuries*," will disappoint those expecting to find a well-finished article. Undoubtedly it has lost much by translation ; and the elegant diction gone, its intrinsic defects are all the more prominent. The reader will save time and avoid vexation by skipping the thoroughly dogmatic, and oftentimes obscure introduction, which would do honor in its pedantry to an ossified German professor of a former period. Much credit is due, nevertheless, to the distinguished French surgeon for having first studied in a systematic manner the relation of injuries to constitutional disorders. Exception will have to be taken to some assertions. It is interesting but altogether doubtful that "the neoplastic diathesis itself is strictly dependent upon arthritism—which is equivalent to saying that neoplastic and cancerous subjects are merely arthritic patients suffering from a special manifestation of the constitutional disease."

The opinion that acute alcoholism forms a positive contra-indication to the employment of anæsthetics, will not be shared by many American surgeons, to whom it is well known that weakly and cachectic patients will stand better a prolonged anæsthesia and much loss of blood, when prepared by a liberal administration of stimulants than otherwise. The parallel drawn between acute alcoholism and diabetes mellitus, in calling the former poisoning by alcohol, the latter *poisoning by sugar*, will at once suggest a further extension of this unphysiological comparison to the state of albuminuria, which might be aptly called *poisoning by albumen*. A strange confusion of cause and effect !

In giving his adhesion to the antiseptic mode of wound-management, the author admits the necessity of modifying his views, as held in the ante-Listerian period ; but he omits to draw a

corollary by stating that wounds and injuries, if received at an incipient period of a constitutional disturbance, that is, at a time when the general condition is not yet gravely impaired, will heal as kindly and readily as those inflicted on a healthy organism. And English and German surgeons have demonstrated that under antiseptic cautelæ even the wounds and injuries of the cachectic victims of syphilis, tuberculosis, and scrofula will heal correctly, often by first intention, as long as accidental wound-complications are excluded by well-known procedures. Of course, this advantage cannot diminish the necessity of a careful medicinal and dietetic treatment of whatever constitutional disorder may be present.

Prof. Verneuil's remarks about the curious effect of injuries in calling forth a renewed attack of a long-dormant constitutional disorder, deserve more attention than the subject has been accorded to thus far by the profession. Much to be regretted is the omission of even a single concrete illustration of some rarer forms of this latter curious process. Throughout the essay no reference is made to the author's large personal experience, or to other observers' publications bearing on this subject.

Professor D. Hayes Agnew's paper on the "*General Principles of Surgical Diagnosis*" is full of valuable hints to the young practitioner. Mature judgment and prolonged experience, together with an agreeable style, enable the writer to accomplish his task in an excellent manner.

Dr. Mansell-Moullin's scholarly paper on "*Shock*" contains all that is worth knowing about this curious form of reflex paralysis. The reader is at once put *in medias res*, and etiology and symptomatology form a pleasant introduction to the pathology of the condition, which may be considered to form the focal point of the short, but thorough and well rounded-off essay. The author's great familiarity with the practical side, as well as with the literature of his theme, deserves all the more credit, as every important fact is carefully verified by reference, and thus may serve as an excellent starting-point for a more extended study of the original authors. It would be hard to add to, but harder still to take away from, the paper one sentence without crippling it.

Dr. Wm. Hunt's article on "*Traumatic Delirium and Delirium Tremens*" reads almost like a nightmare. If the editor intended to introduce a bit of medico-comic writing into the col-

lection of serious essays by way of pleasant change, he undoubtedly succeeded very well. A serious criticism of the article seems hardly possible, and a Pickwickian consideration of the florid, almost poetic style of the author would be certainly very amusing, if not profitable. It must suffice to quote a few examples of the peculiar phraseology and metaphorical style of the paper. A "rare form of traumatic delirium" is thus illustrated by the author in a supposititious case: "The surgeon enters some ward in the morning after a terrible accident has occurred, and finds that a victim of *this kind*¹ has just been brought in and laid upon a bed. He is at once recognized by the patient as one in authority. 'How are you, doctor?' he says, in a high voice; 'what have they brought me here for? I'm not hurt! No, sir! Look at that,' and out goes an arm with the force of a prize-fighter delivering a crusher. 'Look here,' and he tries to lift a leg, which his sensorium falsely tells him he has done, although his expression may indicate a vague and passing doubt. 'Why, there is my wife! Molly, what are you doing here? don't cry; what are you crying for? I'm not hurt; go home to the children, and tell them I'll be there to supper, and at the mills tomorrow. Won't I, doctor? Go home!' Soon this great tension gives way, collapse comes on, and by night the patient is in another home than that in which he promised to be. I have never known such a case to recover."

We find after an enumeration of the well-known causes of traumatic delirium, several pathological conditions gone into extensively, *because they do not cause delirium*, as, for instance, snake-bites, bites of "venomous spiders and insects," hydrophobia, tetanus, and chorea. A chart exhibiting the temperatures of a case of surgical fever with delirium finds its commentary in a pathetic, but very fanciful quotation from "Rab and his Friends." Burns and scalds also receive a quasi-poetical treatment. "I have seen after deep and extensive burns, what seemed to be a living soul looking out of a crisp, charred, and dead body. I have seen a child thus burnt, playing with toys an hour before its death,"¹ etc., and then follow six lines of appropriate verse.

Delirium tremens is also rhapsodized over in the drastic manner of Dr. Hunt, and is fitly branded as "a fiend, sitting as a cormorant hard by the surgeon's efforts, and devouring all his measures for good." Further on we read that "the peculiar tremors of delirium tremens are very characteristic; if I may so

¹ Reviewer's italics.

express it, they are loose and free, without spasmodic jerkings ; somewhat *jelly-like*, and giving a general idea of instability. They seem to say : 'Prop me up, prop me up ; if you don't prop me up I shall die.' " From this the conclusion is drawn naturally and easily, that "propping up" must be the treatment.

The article appears like a huge joke, and we must refer the curious reader to the original.

Professor Henry M. Lyman, of Chicago, contributes a very creditable paper on "*Anæsthetics and Anæsthesia*." The description of the phenomena of anæsthesia is a little masterpiece in its vividity and truthfulness. About two pages of print comprise everything of importance regarding the mortality consequent on artificial anæsthesia. The known proportions of deaths are per centually stated, and the reader is left to draw his own conclusions.

We miss detailed directions for the use of ether and chloroform. So, for instance, the common occurrence of the patient's tongue slipping back on the epiglottis, thus impeding or arresting respiration, and the simple method of projecting the tongue by pushing the lower jaw forward, is not mentioned at all. The disagreeable by-effect of ether in causing very profuse salivation, inspiration of saliva, and occasionally pneumonia, is ignored. The statement of the fact that ether is, chloroform is not, a respirable vapor ; that, accordingly, ether can be administered without an admixture of air, but chloroform ought not, is too important and too suggestive to be omitted. We may add, however, that many of these details bearing on the practice of anæsthesia will be found more fully treated in Dr. John H. Brinton's essay on "*Operative Surgery in General*." This subject being very well known, little opportunity is given to the author to show originality ; but every remark shows the sober good judgment of the experienced surgeon. The commendation by the author of the judicious use of stimulants in the shape of alcohol and opium before grave operations, and his candor in advising the use of chloroform as an anæsthetic in cases of lung trouble in preference to ether, is to be noted. The caution not to apply Esmarch's constrictor to a limb when its muscles are shortened, as in flexion, is valuable, for if the distal portion of the limb be afterward extended, serious injury and laceration of the upper structures may ensue. With some surgeons Lister's antiseptic wound treatment is still *on trial*, and Dr. Brinton belongs to these doubters. It is

only fair to say, however, that he freely admits the beneficial effect of Prof. Lister's labors in transforming ancient into modern surgery. The repetition of subjects disposed of in former papers could have been advantageously avoided.

A number of well-known manuals would have shown to Dr. Charles T. Hunter how to deliver himself creditably of such an easy theme as "*Minor Surgery*" is. Among surgical dressings the omission of the most popular dressing of the period—Lister's—will create surprise. The manifold modifications of Lister's original conception—each of them useful and applicable in certain cases,—such as Bardeleben's moist jute dressing, Thiersch's salicylic acid and cotton dressing, the iodoform dressing as recommended by Mosetig and Mikulicz, finally Neuber's valuable additions to the chapter on permanent antiseptic dressings, are all ignored; but "Revulsion and Counter-Irritation" affords the author an opportunity of enlarging on such important topics as "Issues"—not forgetting even the "*issue pea*"—and "Setons." Commendable is the mention of *Massage*, and its forms of application, a subject undeservedly omitted from almost all hand-books. We fear it must remain a riddle what the sphygmograph has to do with minor surgery.

It is difficult to find a *raison d' être* for Prof. Christopher Johnston's short disquisition, entitled "*Plastic Surgery*"; for the general remarks based on Verneuil's classification would be a suitable beginning to a treatise on special plastic surgery, and are too ponderous to form an introduction to the slim and threadbare subject of skin-grafting. Dr. Frank H. Hamilton's claim to priority in suggesting transplantation is gone into somewhat extensively, and forms the most noticeable feature of the article.

Prof. John Ashhurst, Jr., concludes the volume with a very able and, *sit venia verbo*, carefully written monograph on "*Amputations*." A brief historical sketch is followed by a complete enumeration of the causes calling for amputation, after which the apparatus for blood-saving receive due attention. Among the aortic compressors Lister's is preferred by the author. No mention is made of the new method of compressing per rectum the common iliac, or even the aorta, by the aid of a lever. To diminish troublesome oozing after the removal of Esmarch's band, the author warmly recommends the simultaneous application of a tourniquet with the constrictor. The moment the latter is re-

moved, the tourniquet is brought to bear on the main artery, and very little hemorrhage ensues. It is very regrettable that Prof. Ashhurst has seen no evidence yet to make him think animal ligatures in any way better than the hempen or silken ones. The overwhelming majority of surgeons of the present day seem to have been more fortunate in this respect. Notwithstanding the lessons taught by the experience of the last fifteen years, we find the distinguished author still clinging to the traditions of the long-spent pre-Listerian period. Lister and his disciples are set down as followers of "a fashion," and the merits of the antiseptic treatment of wounds are superciliously disposed of in a few sneering remarks, ill becoming an earnest searcher after truth. The testimony of the author's own cases shows that he never gave this method a single trial, and that therefore his belittling of the accumulated experience of the best men of the period—many of them older veterans of the amputating knife than Prof. Ashhurst himself—is narrow-minded and unjust. Whether "bacteria and micrococci are met and destroyed in their efforts to reach the wound, as in Prof. Lister's dressing;" or "by Mr. Guérin's device are mechanically arrested by a huge thickness of cotton wadding, and being unable to get either in or out, miserably perish in its meshes," is immaterial as to practical results. The upholders of the new departure can well afford to smile at such arguments, when they compare the author's 28 deaths out of 100 amputations (4 at least of pyæmia), with the results achieved by Prof. Volkmann in 261 amputations. Among 73 ablations for injuries, 7 (10.5 %) died of collapse or anæmia within a few hours after operation; of 188 amputations for disease, 4 died of acute anæmia or collapse almost immediately after operation, 1 of tetanus, and 1 of "exhaustion," whatever this may mean, giving a lethal percentage of 3.7.¹ It is necessary to add that all of these were uncomplicated cases, operated on at a time when no septicæmia or pyæmia had set in yet; the 57 cases of the latter order being set apart, as they ought to be, in a distinct table. Here we have 17 deaths versus 40 recoveries, a brilliant showing, considering the exceedingly grave septic processes that had necessitated operation. If all operations done by Prof. Volkmann in five years are thrown together, we see that he has lost 28 out of 318 cases, just as many as Prof. Ashhurst lost out of 100, which means that the Philadelphian's losses proportionately treble those of the German

¹ Oberst, *loc. cit.*

surgeon. Woelfler, of Vienna,¹ supplements this sufficiently conclusive proof in a still more convincing manner. He compares the mortality of Prof. Billroth's uncomplicated cases of amputation done in the same wards of the same hospital, with the same regulations for ventilation and cleanliness, in two numerically very similar groups of cases, operated on in consequence of similar indications, with an exactly identical technique of procedure, *but with a changed manner of dressing*, and demonstrates that whereas the first group of 58 cases (1871-1876) showed a mortality of 16 (21 %), the 56 cases of the second group (1877-1880), treated by the antiseptic method, all got cured, *not one of them died*. Of 704 amputations of all kinds done within the last decade or so by nine German surgeons, 110 were lost, representing a mortality of 15.6 %, a little less than a half of the author's average gained from the amputations of twelve English, French, and American sources. It is noticeable that Prof. Ashhurst wholly, we would almost say intentionally, neglects the enormous clinical material published by German authors. But the battle has been fought and well won long ago, and we must refer doubters to the original sources of information on the subject.

However meritorious Prof. Ashhurst's article may be, it is undeniable that his views and supporting statistics are far behind the rapidly striding times. Newer achievements are simply denied, and the only supports of this negation are a few sarcastic phrases, and a systematic ignoring of recent publications on the subject.

The special amputations of the extremities are very well set forth in a clear and precise manner, and will be found an excellent aid for reference. The hip-joint has received a very exhaustive treatment, the subject fitly terminating in a complete tabulated synopsis of known cases up to the date of publication. A large number of moderately well executed wood-cuts accompany the article.

[A. G. G.]

Minute Structure of the Central Nervous System of Certain Reptiles and Batrachians of America. Illustrated by permanent photo-micrographs. By JOHN J. MASON, M.D. Series A. Author's edition one hundred. Newport, 1879-1882, pp. 31 ; 113 plates.

This elegant and valuable work is entitled to rank first among the neurological contributions which have been issued by the aid

¹ Die Amputationen an der Prof. Billroth'schen Klinik, 1877-1880. Wien, 1882. Braumüller.

of photo-micrography. Indeed the volume seems as near perfection as the arts of printing and photography could make it; evidently no expense has been spared in its publication. It consists of about thirty pages of letter-press, and one hundred and thirteen plates, all photo-micrographs, which are beautifully executed, and, with a few exceptions, remarkably distinct in their details. Those alone who have had experience in the photographic representation of nervous tissues know the difficulties which prevent a satisfactory result in such work: difficulties of illumination and of focusing over a large field, not encountered when small anatomical elements are the objects to be photographed, to say nothing of the labor involved in obtaining sections suitable for the purpose, and a host of minor details. When all this is considered, Dr. Mason's contribution must be regarded as an extraordinary work, and one which could only have been completed by ardent devotion to his subject, involving extended labor, exceeding care, and great expense. The satisfactory results, as evidenced by these beautiful photographs, are, no doubt, a sufficient reward to the author; they cannot fail to be appreciated by all who have any interest in comparative neurology or photography. The production of a work of art is, however, a secondary object with the author; his aim being to furnish a contribution to comparative neurology by a more accurate method than the usual one by means of drawings, where numerous details are omitted and many inaccuracies are introduced from misconception by even the most conscientious and painstaking artist. The author himself admits that "a photograph cannot often show all that can be discovered by more direct microscopic observation," but adds that, "with a judicious working of the fine adjustment, high authority has stated, and perhaps correctly, that a good photograph with a low power—say from 3 to 1-2 inch—is a better means of illustrating the anatomical structure of nervous tissue than hand-drawing." "To give the student enlarged and exact representations of hardened specimens, showing comparative form and dimensions, with as much of the structure as possible, has been the chief aim in view." In the list of animals studied, we find no less than nineteen representatives.

Valuable as is this collection of permanent representations of the central nervous system, its value to the neurologist would be greatly enhanced if more details could have been given, either by descriptions, diagrams, or photographs, concerning the general anatomy of the different animals studied, particularly respecting the

skelital and muscular relations, as they furnish one of the principal keys to the correct interpretation of the functions of the nerve centres. We trust that this will be found practicable for the promised second series. As the author has preferred to make his text principally explanatory, having refrained from presenting many generalizations, reference to the controversies raised heretofore regarding some of the points mentioned would, perhaps, be out of place. It is but just to say, however, that after reiterating his previously enunciated law, that "the nuclei of the so-called motor cells of the central nervous system have, in the same individual, average diameters which are proportional to the power developed in the related nuclei," he adds: "No sympathy with any theory which claims to distinguish between motor and sensory cells is here implied, nor does the writer insist upon any special anatomical importance for the nuclei, as compared with the cell-bodies, beyond their more distinct and regular outline, which makes them very conspicuous, and hence well adapted to accurate micrometric observation. That the nuclei are the true functional centres of the nerve cells is at best an unproved hypothesis; but comparative measurements of these bodies are facts which seem very essential to the future understanding of their function."

[W. R. B.]

The Spinal Nerves. By A. H. P. LEUF, M.D. Brooklyn: F. B. O'Connor, Jr., 1882, pp. 61, 6 charts, 1 diagram.

If there be "no royal road to knowledge," there are at least by-paths which lead more surely and directly to the desired goal than the beaten track pursued by the generality of workers; they are found, however, by those only who are systematic in their search. Much aimless journeying and doubling are done upon the highway of science, with a loss of energy which, properly applied, would have carried the traveller far beyond the point he finally attains. The proper arrangement and classification of a multitude of facts carry us a long way toward the solution of problems concerning them; nor is mnemonics to be despised as an aid to the retention of knowledge. Charts and diagrams can be made to serve such purposes; even smoothing the pathway for the student of that so-called dry subject anatomy. Who renders the attainment of knowledge less difficult is a common benefactor, and Dr. Leuf, in presenting to the students of medicine the publication above-named, merits the gratitude of all who avail themselves of its aid. The author's own language in the introduction sufficiently ex-

plains the plan of the work. "The spinal nerves are treated in three different ways: in the text, by charts, and by a diagram. While studying, the diagram may be pinned against the wall, the charts laid upon the lap, and the text held in the hand, while an illustrated anatomy or atlas should be lying close by for reference. It would, however, be much better to replace the atlas in the closet by the cadaver in the dissecting-room. The topographical arrangement is such that the names of the largest nerves have the largest type, while the name of the divisions and sub-divisions of these are printed in small and still smaller type; the names of the smallest being in italics, thus facilitating reference. The text, except when it is superfluous, is arranged exactly like the charts, the only difference being that each name is accompanied by more or less of a description. The arrangement of the diagram of *all* the spinal nerves *on one plate* is unique, as far as I know."

The amount of labor on such an arrangement is greater than would at first sight appear, and the author gives evidence of having devoted more thought to his subject than is required for a mere compilation. Although the proposal to change old-established anatomical terms is usually considered rather audacious, yet few will consider the substitution of the term "thoracic nerves" for that of "dorsal nerves" as otherwise than a rational one.

The cheap form in which the work is issued brings it within the reach of all students, and if properly placed on the market, will render it a more extensive educator than the elaborate and artistic works of Flower and others which seldom reach the dissecting-room on account of their cost. [W. R. B.]

ORIGINAL OBSERVATIONS.

TWO CASES OF PELVIC ABSCESS TREATED BY ABDOMINAL INCISION. RECOVERY.

By PAUL F. MUNDE, M.D.,

GYNECOLOGIST TO MT. SINAI HOSPITAL, NEW YORK.

Of the large number of cases of pelvic cellulitis and peritonitis which have come under my professional care, but a comparatively small proportion have gone on to suppuration, probably not more than ten per cent., and of these few, some burst spontaneously into the vagina, rectum, or bladder, and others were aspirated through the vagina, the removal of the pus (perhaps several times repeated) being in every instance followed by complete resolution of the exudation, and recovery.

In none of these cases of suppuration did the pus point toward the abdominal surface, with one exception of spontaneous perforation seen many years ago in the lying-in hospital at Wurzburg. During the present year, however, two instances have occurred in my service at Mt. Sinai Hospital, in which the pointing of the abscess toward the cutaneous surface of the abdomen indicated its artificial evacuation at that point, and in which a free opening of the pus cavity resulted in perfect recovery. As these cases are not very common, and one, indeed, presents other complicating features of unusual interest, I will briefly report them.

CASE I.—*Large extraperitoneal plastic exudation (cellulitis) in left half of pelvic cavity; suppuration; free opening of abscess in left groin; drainage into the vagina; accidental laceration of bladder, with escape of urine into vagina and through abdominal wound; spontaneous closure of fistula; complete recovery.*

M. G., married, æt. thirty years, admitted May 18, 1882. Mother of six children, the last, twins, seven weeks previously,

after which she was seized with a chill, high fever, and severe pain in the left hypogastrium which continued for several weeks, part of which time she was delirious. About five weeks after confinement she first noticed a large swelling in her left groin, which, on admission, showed distinct fluctuation on palpation and bimanual examination. The general condition of the patient was very poor; she was exceedingly anæmic, occasionally slightly delirious; temperature, 101° ; pulse, 110, very feeble. Hot poultices to the abscess; stimulants and quinine were ordered, and sufficient opium to relieve the constant pain.

May 22d.—Patient somewhat stronger, pain less; temp., 100.6° . The swelling somewhat larger, extending toward the median line over the bladder.

May 25th.—Aspiration and removal of two ounces of pus, and subsequent small incision, with evacuation of at least a pint of pus. A long sinus was discovered leading toward the bladder and across Poupart's ligament. This was opened to the length of three inches, and the wound syringed out and packed with carbolyzed cotton.

June 8th.—The patient's temperature rose to 104° ; pulse, 120, evidently in consequence of imperfect evacuation of pus. A long probe revealed a sinus extending transversely one inch across the median line immediately above the pubis, another some two inches upward into the iliac fossa, and a third, bifurcated, down into the pelvic cavity so that the point of the probe could plainly be felt through the anterior vaginal wall. As it was evident that thorough drainage was imperative, the patient was chloroformed, and the sinuses were freely laid open on the finger and director. The wound measured at least six by four inches and four inches in depth. A long director was then passed downward and the anterior vaginal wall gently pressed out until the corresponding spot appeared at the vulva, when a small opening was made directly on the point of the director, and the latter pushed through. A fine rubber drainage tube was then tied around the director, and with a rapid motion drawn into the sinus and out of the abdominal wound. As I drew the tube through, I noticed a slight obstruction at the lower orifice of the wound, which immediately yielded, but simultaneously the wound filled with a clear fluid, which was readily seen to be urine, and which also gushed from the vagina through the drainage tube. Evidently the bladder had been adherent to the wall of the abscess, its coats had become friable by long contact with pus, and, perhaps, by plastic infiltra-

tion, and the slight force exerted in drawing through the drainage tube ruptured it. No attempt was made to ascertain the size of the fistula; the drainage tube was left in place, the wound packed with carbolized cotton, and hot poultices ordered, to reduce the extensive infiltration about the abscess. The bladder was directed to be washed out three times daily with a solution of boracic acid, one drachm, to the pint of tepid water, through an elastic catheter, which was left in the bladder. Stimulants, *ad libitum*.

June 9th.—Temp., 101° ; pulse normal; wound looking well.

June 10th.—Temp. during last three days between 101° and 102° , but the patient feels and looks stronger and better. The fluid injected into the bladder flows freely out of the abdominal wound.

June 22d.—Patient's condition has not been so favorable during the last few days, apparently in consequence of fresh exudation in the iliac fossa. As hot poultices had been continued all this time, the effect of ice in restraining this spreading of the exudation was tried and with great success. Within twenty-four hours the temperature fell to 98.5° , the pain ceased, and the hardness and tension of the abdominal walls disappeared.

The drainage tube was now removed, as the wound was doing nicely, but the injections of two-per-cent. carbolic solution into the wound and of borax into the bladder were continued, and Peruvian balsam was poured into the wound as a gentle stimulant. The patient slowly gained strength and flesh, and when I saw her, on my return from the country, early in August, had grown almost stout and looked perfectly well. The permanent catheter was worn for about six weeks, until it was noticed that the bladder injection fluid no longer escaped by the abdominal wound, when it was found that the fistula had entirely closed, and the bladder had regained its normal function.

Sept. 3d.—Abdominal wound almost entirely closed. Patient stout and well and anxious to return home. Bladder in perfect order. Discharged.

CASE 2.—*Cellulitis in left broad ligament; abscess; free abdominal incision; recovery.*

A. L., thirty-three; married; admitted, July 10, 1882. Tedious confinement four weeks previously. On tenth day had a chill followed by high fever and pain in left inguinal region and pelvic cavity. Two weeks before admission, noticed a slight swelling at seat of pain, which is perfectly distinct on admission, and on palpation and bimanual examination shows fluctuation. In the left broad ligament can be felt a large mass, evidently parametritic deposit. Temp., 101.4° .

July 11th.—Abscess opened by small abdominal incision, and about eight ounces of pus evacuated. A sinus was discovered leading toward the symphysis pubis, near which point a counter-opening was made, and a drainage tube introduced. Poultices and tonics were ordered. Temp. normal next day.

July 17th.—Patient better, but sinus gives no evidence of healthy action although washed out daily with two-per-cent. solution of carbolic acid. Appetite good ; temp. normal.

Aug. 4th.—As the sinus still refused to close, and was discharging thin, serous fluid, it was thought best to lay it thoroughly open, which was done by knife, on a director, inflicting a wound some six inches in length. There was considerable arterial hemorrhage, since the superficial epigastric and several small twigs were divided. Considerable difficulty was experienced in seizing the bleeding arteries because they were so deeply seated in the infiltrated tissue, but finally the larger branches were tied, the wound was washed with five-per-cent. solution of carbolic acid, and tightly packed with sheet lint, when all hemorrhage ceased.

Aug. 5th.—Patient doing very well. Poultice discontinued, and balsam of Peru poured into the wound, which was packed with dry oakum.

Aug. 11th.—Wound rapidly filling by granulation ; patient much stronger. Parametritic deposit is still hard, but smaller than when admitted. Tonics.

Aug. 24th.—Wound almost healed and deposit gradually disappearing.

Sept. 10th.—Wound entirely healed ; exudation only as large as a small walnut, but still hard. Patient feels very well. Discharged.

I do not propose entering into a lengthy discussion, either of the etiology or pathology, or of the principles of treatment, of pelvic abscess. I would merely say, that whenever such abscesses point toward the abdominal surface, they should be opened and treated precisely like similar collections of pus in other parts of the body. When there are several sinuses and openings, it is always best to split the former and unite the latter, even though the wound thus made may appear very large, since thorough drainage and cleanliness of the cavity, and rapid healing are undoubtedly promoted by such free incisions. When fluctuation can be felt with equal or even greater distinctness from the vagina, I think it unquestionably the better practice to aspirate, or, if neces-

sary, incise there, since a cutaneous wound is avoided, and natural drainage is thereby at once established.

As regards the accidental production of a vesical fistula in case 1, I hardly see how it could have been foreseen. Still, in a future similar case, I should endeavor to avoid the accident, making the counter-opening at once so large as to easily admit the drainage tube. The spontaneous closure of the fistula was due to the constant withdrawal of the urine through the catheter, and to the frequent cleansing, and antiseptic injections made by the house-surgeons, Drs. Cramer and Rice, to whose unremitting care, in a great measure, the recovery of the patient is due. Relying on the statement of Dr. Emmet, that an artificial vesical fistula (such as is made as a cure for chronic cystitis) can scarcely be kept from spontaneous closure, I had from the very first assured the patient that the rent in the bladder would close without operation, although, I confess, that I did not feel quite as sure of that result as I pretended to be.

CASES ILLUSTRATING THE ACTION OF CONVALLARIA MAIALIS ON THE HEART.*

By ANDREW H. SMITH, M.D.

CASE 1.—A young lady, aged sixteen, in the third week of typhoid fever. An irritable stomach had made efficient alimentation difficult from the first, and although the temperature was never extreme, reaching only once to $104\frac{1}{2}^{\circ}$, and varying from $101\frac{1}{2}^{\circ}$ to $103\frac{1}{2}^{\circ}$ a day, adynamia occurred early, and the pulse showed a frequency out of proportion to the temperature, ranging between 120 and 140. The respiration was also frequent, not falling below 30 after the first week, and toward the beginning of the third week rising to 40. Repeated examinations of the chest failed to elicit any morbid physical signs.

Brandy freely administered produced remarkably little effect either upon the circulation or the sensorium.

Nov. 15th, being toward the close of the third week, and two days before death, the pulse was 144 in the morning, and by the time of my visit at 6 P.M., had reached 150, and was so feeble that it was very difficult to count it accurately. The first sound of the heart, though distinguishable, was very faint. Five minims of the fluid extract of convallaria, prepared by Parke, Davis, & Co., had been given three or four hours before, but it had been immediately vomited. At six o'clock, four minims were adminis-

* Read before the Section on Practice of the Academy of Medicine, Nov. 21, 1882.

tered hypodermically. At nine o'clock the pulse was still 150, but notably fuller and more distinct. The trained nurse stated that she observed a change in the quality of the pulse about an hour after the injection was given. The respirations were 50 per minute and very superficial. First sound of heart very faint. Lips blackened with sordes; face dusky.

Dr. C. W. Packard, who was present in consultation, having concurred in the propriety of repeating the dose, six minims were injected at 10 P.M. At 10:15 the pulse had fallen from 150 to 128, and was distinct, but irregular. At 10:20 it was 120, strong and regular. Five minutes later it was 108, and soon after it touched for a moment 98. Both heart-sounds were almost preternaturally loud, and the cardiac impulse was plainly felt. The respiration, however, remained at 50, but was somewhat deeper. The face lost its dusky hue and assumed a fresh and natural color. There was no appreciable effect upon the sensorium, the delirium continuing as before; subsultus and picking at the bedclothes, which were present, also remained unchanged. At 10:35 the maximum effect was obtained. The pulse from that time began to rise again, and at 11 o'clock was 126. The respirations increased to 60. From these figures there was again a decline, and at 7 o'clock the next morning the pulse was 102 and the respiration 48. The sordes had disappeared from the lips. The temperature showed but little change, remaining between 102 and 103. There was a good deal of nausea during the night, and several attempts to vomit. The urine, which all along had been abundant, was not increased.

The local effect of the injection was limited to the production of a small erythematous patch, which remained until death.

In the course of the day the pulse increased to 120, but still retained a fair amount of force. During the night, however, the coma deepened, and it became more and more difficult to administer nourishment or stimulants. Mucous râles developed in the lungs, the radial pulse could scarcely be distinguished, and by 7 o'clock in the morning of the 17th the patient was plainly moribund. Yet, even then, both heart-sounds were clear and distinct. Death took place at 9 o'clock A.M.

It would seem that the action of the convallaria, in this case, at least, was the reverse of what is observed when the pneumogastrics are divided. This operation results in quickening the heart's action and slowing the respiration, while the convallaria slows the pulse and quickens the breathing.

This case confirms, in the main, the observations of Prof. See and those made in 1867 by Dr. Marmé, of Göttingen.¹

CASE 2.—Called, in consultation with Drs. A. S. and A. E. M. Purdy, to see a patient at the Windsor Hotel, Nov. 20, 1882, at 7:30 P.M.

The case was one of peritonitis, in a gentleman about 35 years of age. He had been taking a grain of opium every two or three hours, and at half-past four was bright and intelligent when spoken to, showing nothing more than a slight drowsiness. At six he was found in a stupor, from which he could not be roused. Dr. Purdy was called, and found him unconscious, with limbs rigid, a good pulse, and breathing fourteen times a minute. Respiration, however, soon became very superficial, and in the course of a few minutes apparently ceased. Artificial respiration was resorted to, and a battery was employed. Breathing was restored after nearly half an hour's labor, but it was very shallow, catchy, and irregular. At this time I saw the patient. He was breathing so imperfectly that compression of the chest was frequently required to excite a respiratory effort, which would then be followed by half a dozen quick, jerky superficial respirations. The face was livid, the mouth open, conjunctivæ scarcely sensitive, pupils moderately contracted; pulse very rapid and irregular, at times large and gaseous, then flickering and scarcely perceptible. The heart's action was a confused wobbling, in which no distinction of first and second sound was possible.

Although the picture was so little that of opium poisoning, it was thought well to administer atropine and coffee, as both are heart tonics, and were indicated irrespective of the question of opium narcosis. Atropine and caffeine were given hypodermically, and half a pint of black coffee was thrown into the rectum. No result having been obtained in the course of three quarters of an hour, the symptoms showing no improvement, it was determined to try the effect of convallaria. Eight drops were injected under the skin at 8:30, the pulse then being 150 and exceedingly feeble; the respiration varying from 30 to 44; the inspirations quick and sibilant, the expirations moaning. Face deeply livid; lips and tongue almost black. A specimen of urine was obtained by passing a catheter, and was found to contain albumen. At nine o'clock the condition was unchanged, and eight drops more were injected. In the course of fifteen minutes the pulse fell to 135; five minutes later it was 125; at the next counting it was 120,

¹ *N. Y. Med. Journal*, Nov., 1867.

full, strong, and regular. The respirations became fuller, and slower, and more regular; the lividity gradually disappeared from the face; the lips became red; the eyelids moved when touched; the mouth closed when a spoon was put to the lips; soon the hands were moved; the eyes opened when the head was lifted up; intelligence began to return, and when asked by his mother if he knew her, he replied yes. By half-past eleven consciousness was fully restored, and the patient was able to converse.

As the action of the heart grew more steady, a harsh murmur was heard with the first sound at the base. On inquiry of the family, it was ascertained that for some time he had been short of breath on going up stairs.

At nine o'clock this morning (21st), barring headache and some abdominal tenderness, he was perfectly comfortable. Pulse 108, temperature $101\frac{3}{8}^{\circ}$.

TWO CASES OF GLYCOSURIA; ONE TRUE AND ONE SIMULATED.

By E. C. SEGUIN, M.D.

The two following cases are presented because they are both in some way unique.

CASE I.—Mrs. ——— consulted me in October of this year for simple, moderate dementia. She was fifty-one years old, and had masturbated for years. Six years ago she had an attack of quite acute melancholia.

In the last three years marked emaciation had taken place. No positive delusions or hallucinations seemed to exist. The memory had been fairly retained.

The patient fancied that her shoulders were paralyzed, and wanted to have her arms cut off. There existed no actual paralysis. The patient's manner was demented and hypochondriacal. She had no symptoms of diabetes except the emaciation, hence it was not suspected.

As a matter of routine her urine was examined with the following result. An afternoon specimen had the specific gravity of 1045, and contained a trifle over five per cent. of sugar, but was otherwise normal. A specimen passed the next morning had the specific gravity of 1018, and did not contain a trace of sugar.

October 14th.—Two days later, a morning and night specimen were sp. gr. 1020 and 1011 respectively, and contained no sugar.

Oct. 16th.—The urine had the sp. gr. of 1019 and 1023, and was free from sugar.

Oct. 21st.—Four specimens were found of normal specific gravity and reaction. When last heard from (about November 12th) the patient was doing well and had no symptom of diabetes. The patient had neither medication nor diet which could have caused the sudden disappearance of sugar from the urine.

Cases of mild diabetes are not rare where an occasional sample of urine is found devoid of sugar ; but a fall in the specific gravity from 1045 to 1018 and from five per cent. of sugar to none in twenty-four hours, is by no means common.

How long the patient had been passing sugar cannot be told, and we may perhaps look on the case as one where some peripheral sensory impression or some transient central alteration produced a disturbance of the chylo-poietic circulation which interfered in proper glycogenesis. This is rendered more probable because Pavy, Cyon, Aladoff, Schiff, and Eckhard have shown that irritation or destruction of many parts of the peripheral nervous system will, in a reflex way, produce glycosuria, while by puncturing the floor of the fourth ventricle, Bernard long ago caused sugar to appear in the urine of animals. Among diabetics, too, a profound mental or bodily shock almost invariably causes an increased excretion of sugar.

CASE 2.—In May of this year an old patient of mine sent three specimens of urine, with the request that they should be thoroughly examined. He said they were passed by a niece of his, who was under the care of a homœopathic physician for some hysterical affection. More definite data than these it was difficult to obtain. A night specimen of May 18th. was pale, cloudy, contained some uric acid, and a *trace* of sugar. It had the sp. gr., however, of 1070 ! A specimen passed the next morning contained only a trace of sugar, and had the sp. gr. of 1048. A noon specimen of that day was pale, poured like syrup, and, with only about three per cent. of sugar, had the sp. gr. of 1090 !

Besides these examinations, which were made by Dr. Amidon, I sent the heavy specimen to Mr. Charles Rice, of Bellevue Hospital, and the following is an extract from his letter in reply :

“ There is, however, something present, which I cannot make out exactly, but which I have separated and find to resemble in some of its properties ‘peptone,’ or digested albumen. If a portion of the urine is mixed with about five times its volume of absolute alcohol and shaken, a gummy substance separates, which may be caused to agglutinate into a lump by judicious turning of the test-tube. After being washed with alcohol it is found to be but little

soluble in boiling water, but it dissolves in cold water, particularly after addition of a drop or two of hydrochloric acid, to a very thick, almost colorless, faintly opalescent liquid. It gives no reactions with nitrate of silver, or mercuric chloride, and does not exhibit any reactions of the ordinary gums."

June 6th.—Two more specimens were sent and examined by Dr. Amidon and Mr. Phelps. A night specimen was clear, pale, and gave a faint sugar reaction. It was syrupy, and had a sp. gr. of 1105! A morning specimen, while it seemed to contain less sugar, had a sp. gr. of 1117!! A fraud was immediately suspected, and its detection was attempted.

Mr. Rice kindly examined the latter specimen and said: "The last specimen of 'urine' seems to contain but little genuine urine, to judge from the small amount of the sulphates, phosphates, chlorides, and urea present." "It contains a body (or mixture of bodies) insoluble in alcohol, and partaking of *some* of the properties of starches and of gelatin, without, however, giving a reaction for starch itself, or a definite reaction for gelatin." "I should say the thing is a fraud."

A series of experiments were then made with gelatin, cane sugar, and various syrups. It was finally found that a mixture of a fine sorghum syrup and urine, in such proportions as to bring the specific gravity up to about 1115, produced an exact imitation of our puzzling specimen. It gave the same precipitate to absolute alcohol, and contained the same amount of grape-sugar. It was, moreover, found by Dr. Amidon, that on standing and fermenting the cane sugar was transformed into glucose and gave a magnificent reaction to Fehling's test, while, when fresh, only a faint sugar reaction was obtainable. Also it was noticed that the specific gravity of the specimen of June 6th, which had stood in the laboratory uncorked for ten days, had fallen from 1117 to 1010, showing that the cane sugar had fermented and disappeared.

The mother of the patient was asked to *see* some urine passed in a clean vessel and send that for examination.

It was some time before this could be brought about, but at last a night specimen of urine was sent in which was clear, of a reddish-yellow color, acid, free from albumen and sugar, and of the specific gravity of 1024.

NOTE.—Copies, on heavy paper, of the cuts in Dr. Seguin's article, page 260, can be obtained of the publishers.

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